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After the addition of a ferrule the next step was to increase the width of the tang near to its junction with the blade so as to allow of rivets being passed through in order to attach the ferrule to the tang (Fig. 5). The shaft, however, in spite of the strengthening effect of the ferrule was necessarily so weakened by the insertion of the tang and the consequent thinness of the wood left between the tang and the ferrule that it became too slight to sustain the strain made upon it when in use. It was therefore necessary to make a change, and this was effected by doing away

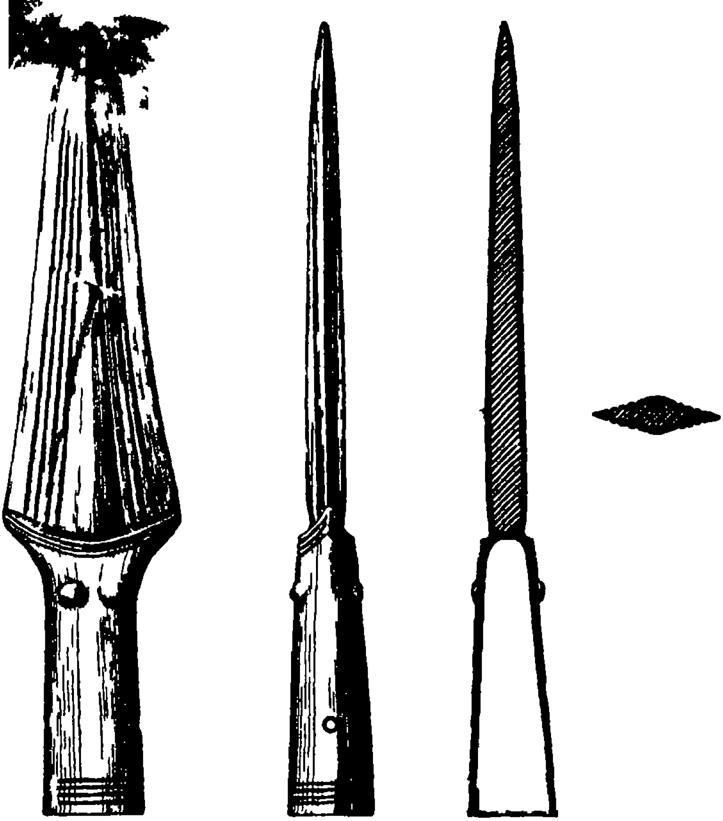
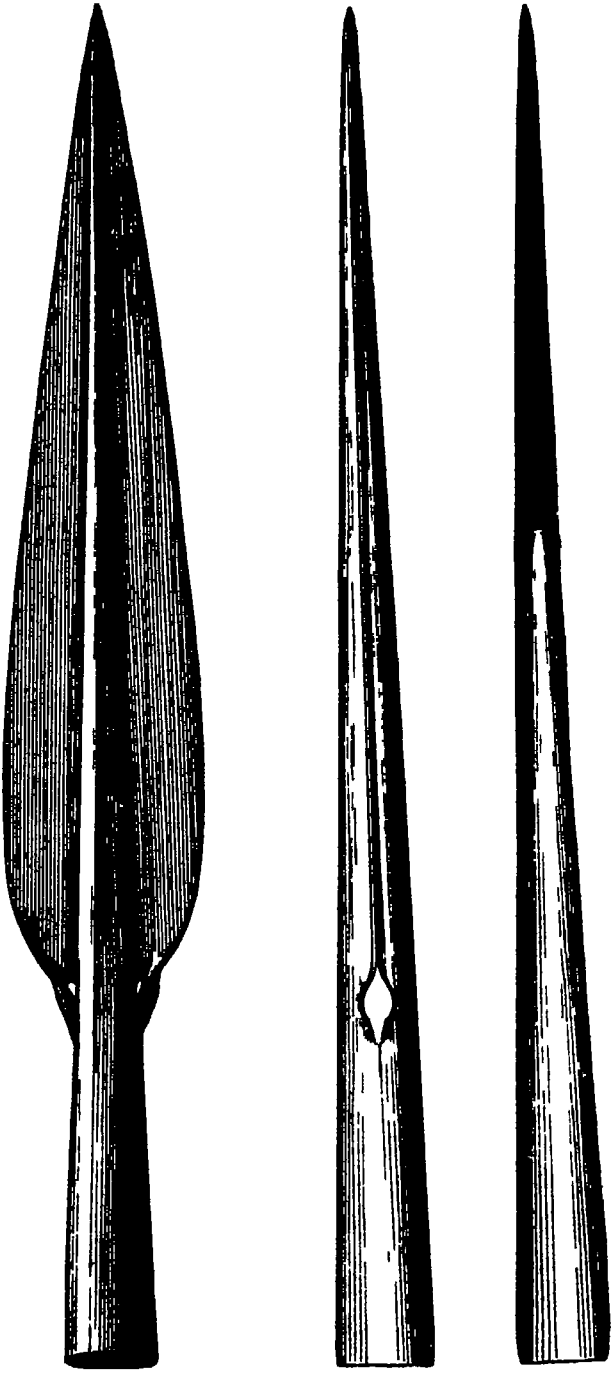


Fig. 6.—Arreton Down. [B. M.]

with the tang and casting the blade and ferrule in one piece, a much superior mode of hafting than had hitherto been adopted. This very important step, the union of the blade and ferrule, marks a distinct stage in the evolution of the spear-head. It is in reality the invention of the socket, and though it involved a great change, and even introduced a new principle, it nevertheless came about by a very simple process The head shown (Fig 6) is formed by casting the ferrule in one piece with the blade. It was found in the Arreton Down hoard, and may be regarded as the earliest known socketed bronze spear-head which has been discovered in the United Kingdom It has no loops, but has been attached to the end of the shaft by means of a pin which passed through two holes and the intervening shaft This constitutes the earliest mode of fastening the socketed spear-head

In the Arreton Down head, as also in those which immediately succeeded that form, the cavity of the socket does not extend into the blade but stops at the line of the simulated mouth, where the blade and socket met before they were cast in one piece. The simulated mouth of the socket, and the simulated rivet heads, are no doubt derived from the true mouth and rivets of the Snowshill type of head, from which the Arreton Down head differs in having the tang omitted and the blade and socket cast in one piece. A comparison of these two heads, Figs. 5 and 6 will show clearly that the Snowshill head is the prototype of that from Arreton Down even in minute details. The socket of the spear-head was therefore derived directly from the ferrule of the tanged type. It is a remarkable fact that the invention of the socket as a method of attaching the head of the spear to the shaft anticipated, apparently by a long period, the same provision for hafting the axe. When once the socketed spear-head was adopted it must have become apparent that it was the best method of attaching the blade to the shaft, and it continued to be used in that relation during the remainder of the Bronze Age, and though various modifications took



the sloping outline of the sides of the blade (Fig 13) They ultimately move higher up and become what are called the

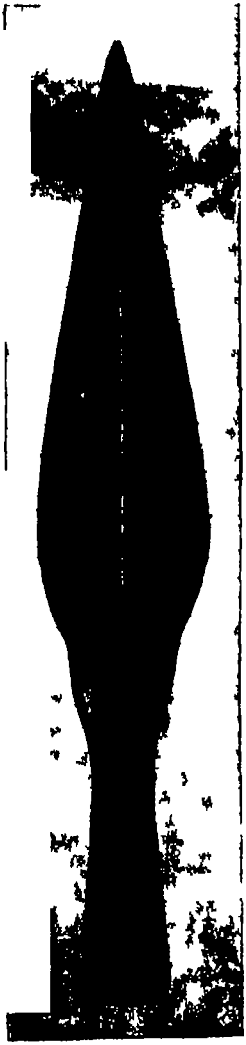


Fig 12 Twickenham [B M]

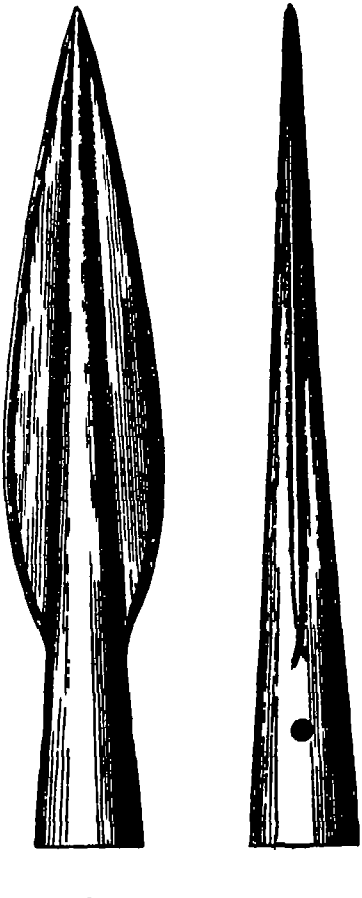


Fig 14—Scunthorp Lincs [B M]

Protected Loops The angular outline has now quite disappeared and the edge flows in one unbroken line from its point down to the base where it joins the socket. The wings themselves have become plain and of almost uniform thickness.

The latest development in the relation of head and shaft took place when the method of fixing the shaft by a peg or pin was reverted to (Fig. 14) and the loops had entirely disappeared, or only survive in the piercings which occur in some of the heads in the form of lunate and other openings.

The full development has now taken place, and the spear-head had passed into the leaf-shaped socketed type, which, with various modifications and differences in subordinate particulars, prevailed down to the end of the Bronze Period..

The progress in the development of the spearhead had throughout been towards simplicity and efficiency, and this was carried out so fully in the leaf-shaped heads that there appears to be no further room for improvement in that direction. Henceforward any advance that was made was by means of economy in the use of metal, which at the same time reduced the weight All these methods seem to have been in use at the same time, and to have continued until the end of the Bronze Age.

One of the new methods which came in with the early leaf-shaped heads was by piercing the wings with openings at their thickest part where they abut on the midrib socket (Fig. 15). These openings are frequently lunate in shape, for that form is the best mode of lessening the amount of metal. The straight side of these openings is placed next the midrib socket, while the curved side follows more or less the outline of the edge of the wings. These lunate openings not only economise metal and reduce weight, but also add to the appearance of the heads. In some of the smaller heads the openings are merely circular holes (Fig. 16). In others, especially in those of more than ordinary size, the larger opening is now and then supplemented by the addition of small circular holes in the nar-

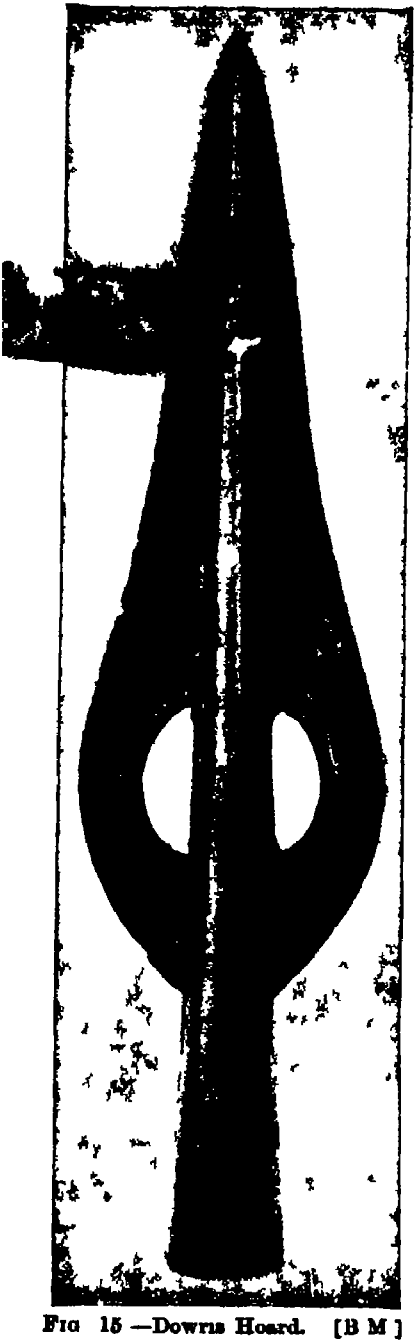


Fig 15 —Downis Hoard. [B M]

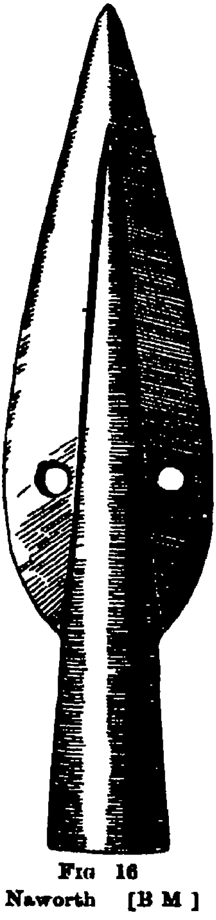


Fig 16

Naworth [B M]

rower portions of the wings above and beneath the principal opening. Heads with lunate openings are not uncommon in England, Scotland and Ireland, but are very rarely found outside these countries.

Another method of lessening the amount of metal, which only came in with the later leaf-shaped sheads, was that of making the whole head hollow In the late leaf-shaped heads there is a gradual merging of the wings into the midrib socket. This feature, which produced a thickening of the wings as they approach the midrib, afforded room for extending the socket-cavity into the wings, thereby removing what would otherwise have been superfluous metal. This hollowness, which began by a very trifling extension of the socket-cavity, was finally carried to such an extent that many heads are merely shells, with walls in some cases less than one-twentieth of an inch in thickness. In these heads with thin walls the midrib is frequently absent, or only represented by a narrow bead, Fig. 17 There appear to be no hollow heads outside the United Kingdom, because in foreign examples the wings are not thick enough to allow of their being made hollow.

From its first inception throughout the whole progress of the evolution, till it finally culminated in the hollow-head, the spear-head of the United Kingdom has a character of its own, one quite different from those found elsewhere. No other country can show such a sequence of forms as we have, a fact which at least seems to claim for these islands an indigenous bronze spear-head.

THE SWORD.

The sword is essentially a metal weapon, *i.e.*, a wooden weapon is at best but a flattened club, and not until some considerable time after the advent of metal did the true sword come into existence. Its origin is to be found in the copper or bronze knife with a small thin blade, which in the United Kingdom is represented by the so-called knife dagger, which in its earliest form has a rounded point, and

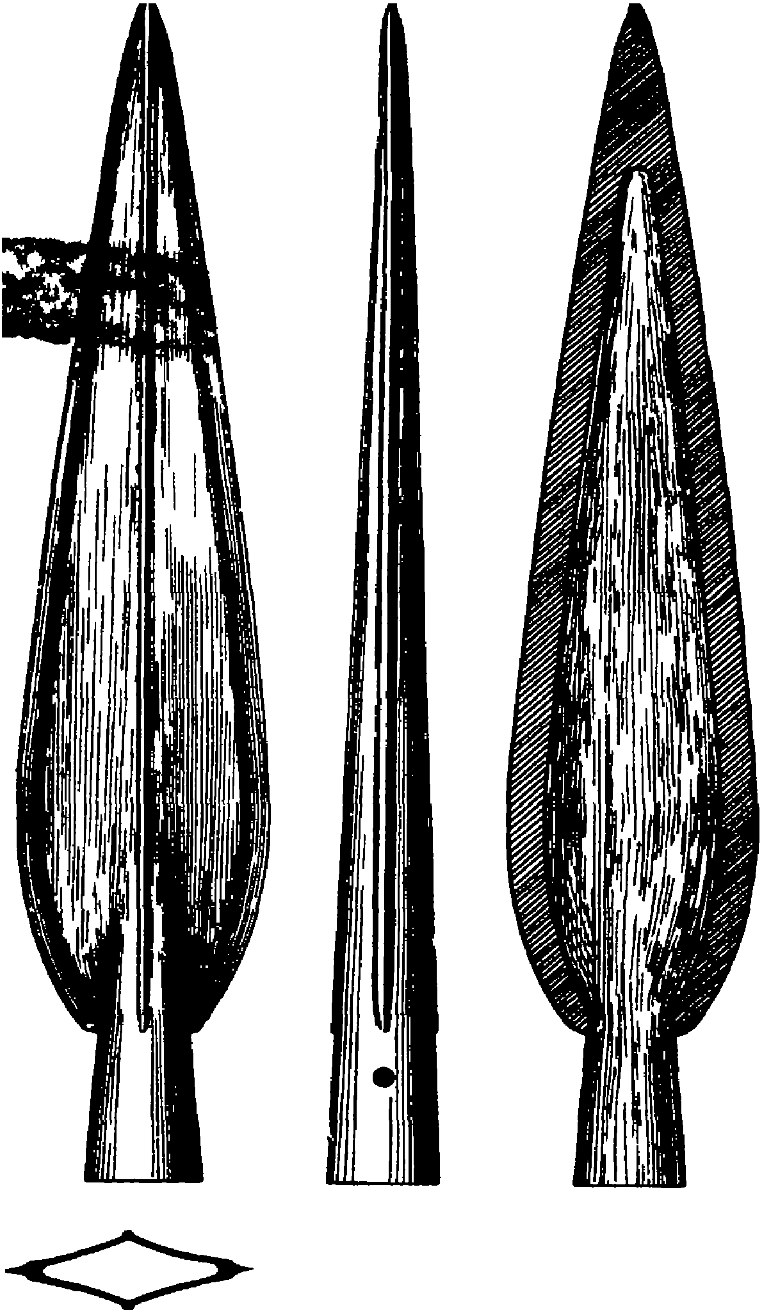


Fig 17 River Thames at Richmond [B M]

was probably a domestic implement rather than a weapon (Fig 18) This implement is found in the early burials and belongs to a time when metal was scarce but as it became more plentiful the blade was thickened and given a midrib

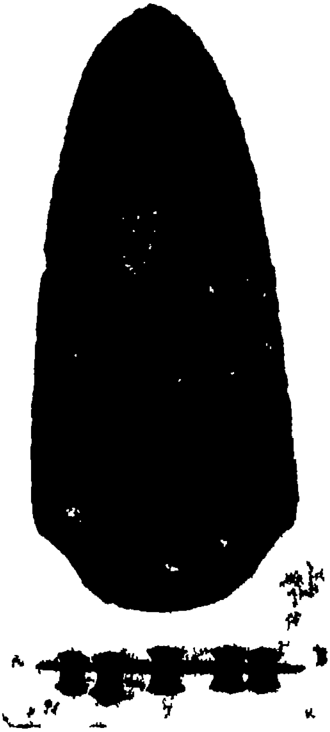
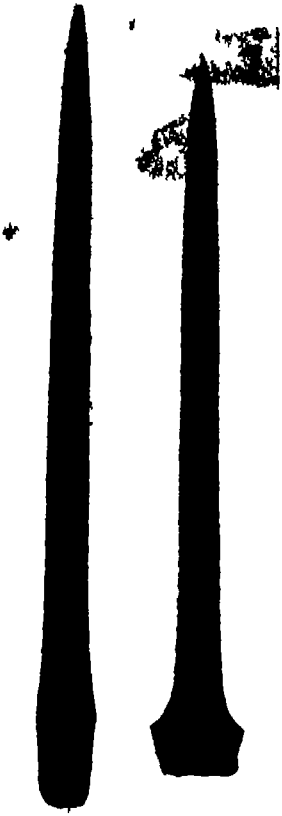


Fig 18



Figs 20 and 19

and point It had then developed into the true dagger for warlike purposes well adapted to thrusting but less so to cutting (Fig 2) With increasing command of metal the dagger was elongated into a thin tapering weapon special used for thrusting, which has been called a rapier (Fig 19) These bronze rapiers display very considerable skill in design and manufacture, some of them being over thirty inches in

Fig 21 —River Tyne
[Blackgate Museum]

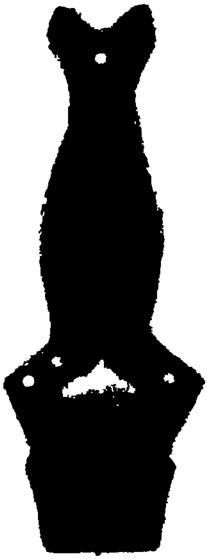


Fig 22 —The Hilt Plate
of Fig 21

length, and only five-eighths of an inch across the centre of the blade, excellent weapons for thrusting But thrusting is an acquired art and man's natural blow sweeps in a semicircle, so that in the heat of a fight he instinctively slashes at his enemy, and in time the rapier was displaced by a sword which ultimately expanded into a broad leaf-shaped blade adapted for cutting.

These leaf-shaped blades were at first mounted similarly to the daggers, but the rivets near the edge of the base of the blade were frequently torn out, for a much greater strain was thrown on them in cutting than in thrusting, so an alteration in the method of fixing was necessary, the tang was gradually extended into the hilt (Fig. 20) until it became a plate sandwiched between two other plates of horn or bone, (Fig. 21) which form a much more secure method of attachment and eventually became the popular method of the Bronze Age.

Many writers have commented upon the very small size of these sword hilts, and argue therefrom that the men who used them had very small hands and therefore were men of less stature than men of the present day. But this is entirely an error. We know from the skeletons tabulated by Dr. Greenwell that the average Yorkshireman of that day was about five feet nine inches in height. The explanation is as follows (Fig. 22) You will observe that there is a notch on either side of the base of the blade, which is intended for the first finger, and if held in this manner the hilt is quite large enough for the hand of an average man.

The leaf-shaped spear and sword are the forms par excellence of the British Bronze Culture. They are admirable alike for design and workmanship. They may have been equalled, but certainly not surpassed, by those of any other part of the world.

THE INFLUENCE OF CONDUCTIVITY ON THE APPARENT DIELECTRIC CONSTANTS OF LIQUIDS.

By Professor W. M. THORNTON, D.Sc., D. Eng.

[Read November 14th, 1912.]

The electric polarisation of a perfect insulator has two components, a true displacement of the æther and a separation of electrical charge within the molecule. There are no free electrons or ions and there is therefore no conduction current. In an imperfect or conducting dielectric there is also a transfer of charge generally assumed to be electrolytic in type. It is however difficult in such a case to discriminate between dielectric polarisation in which, whilst the charges are separated in the molecule, they do not escape from it, and the ionic conduction of electrolytes. In steady fields the final current is of the latter type, but at the start and wherever there is time variation of voltage the polarisation terms must also be considered.

The behaviour of a conducting dielectric may then be examined by direct measurements with steady currents or by observation of capacity currents in alternating fields. The difficulty in the former case arises chiefly from chemical polarisation at the electrodes, and in the latter from the fact that any expression for the charge on the boundary surface of such a medium includes the product of the resistivity and the true dielectric constant.

In a condenser composed of flat parallel plates of dielectric having a voltage gradient V through them, the current density is $-V/\rho_1$, the negative sign denoting that the voltage is falling in the direction of flow. But at a surface separating two media of dielectric constants K_1, K_2, and resistivities ρ_1, ρ_2, the density of charge which gives the apparent dielectric constant is, by Gauss' Theorem, $\sigma = \frac{(K_1V_1 - K_2V_2)}{4\pi}$, V being measured from the interface in each case. Thus $4\pi\sigma = -(K_1\rho_1 \quad K_2\rho_2)\iota$. If the second medium is a metal in which the true polarisation coefficient K is unity and ρ very small, the charge is proportional to the product $K_1\rho_1$, and to the current density ι at any instant. The product $K\rho$ is of great practical importance in the transmission of signals along wires embedded in dielectrics, appearing as the so-called Kr law.

The apparent conductivity, that is the ratio of the current to the voltage, of a condenser having a capacity K and a perfect dielectric, is $s = 2\pi n K + dK/dt$. For since $q = KV$, $\iota = K\dot{V} + V\dot{K}$, and $s = \iota/V = K\dot{V}/V + \dot{K}$, which when V is a simple harmonic of frequency n, has the above value, each term of which is a function of the time.

It is the more difficult to distinguish between the effects of the K and ρ components in the case of a conducting dielectric, for both may be influenced by change of frequency and by changes in the composition of the substance.

Influence of frequency.—Most of the measurements of K in liquids have been made with the liquid in contact with the terminal plates of the condensers, and there would appear to be a difference introduced by not making direct contact with them. Thus the values given in Winkelmann (Band IV. p. 144) for the dielectric coefficient of water, which are the mean values of many observations by such contact methods, are:

n	$< 10^4$	22.10^5	50.10^6	$1.5.10^8$	$8.2.10^8$
K	80.1	77.9	80.82	80.8	82.5

Within the limits of experimental error the dielectric coefficient here remains constant over a remarkably wide range of frequency. But when water is enclosed in a quartz ellipsoid as used by Beaulard (Science Abstracts, 1905-7-8 and 1910) suspended in a Hertzian field so that there is no contact between the electrodes and the liquid, K is found to decrease in a consistent manner as the frequency is raised. Beaulard's values may be summarised as follows:—

n	$8{\cdot}3.10^6$	$11{\cdot}10^6$	25.10^6
K	3·32	3·31	2·78

For ice first melting K = 1·455 and for water just freezing K = 3·072, at $n = 6.10^6$. The square of the refractive index of ice is 1·71 which agrees fairly well with the dielectric constant.

The alcohols have dielectric coefficients ranging from 14 to 30 measured at long or short wave lengths by any of the usual methods; but when enclosed as above in a quartz ellipsoid these fall to 4·56, for example, at $\lambda = 35$ metres, or 3·70 at $\lambda = 12$ metres.

It is clear from this that the dielectric constants of liquids other than electrolytes are affected by arranging the experiment so that no conduction or quasi-conduction currents can flow. In the case of a conducting liquid suspended inside an insulating ellipsoid the resultant torque is only that of the true dielectric polarisation, for any movement which gives rise to charge on the inside ends of the containing vessel induces an equal opposite charge on the outside surface, and since every precaution is taken by drying to make the outer surface non-conducting, these induced charges cannot combine and so remaining mask the entire effect of the inside separation which indeed can only take place freely when the flow is circuital. Havelock has shown that electrical double refraction in liquids can be accounted for by electrical distortion of the molecule from a sphere to a spheroid. There would therefore appear to be a true elastic displacement of the electronic charge in the molecule from a spherical to a spheroidal configuration which has a minimum value at the velocity of light and has an amplitude several times greater than this at very low frequencies. The values obtained by Beaulard are intermediate, but approach the lower limit. The square of the mean of the refractive indices of a number of alcohols for the D line is 1·96, and there is dispersion to be considered

Influence of added salts.—The position of a conducting dielectric in the scale of resistivities cannot be defined sharply. "Conductivity water" at one end of the range has a resistivity of 10^6 ohms per cm cube. For saturated sodium chloride solution in water ρ is 4·77 The range within which liquids can be classed as conducting dielectrics may be taken to be between 1 and 10^6 ohms per cm. cube.

In a perfect insulator the dielectric constant is the square of the refractive index μ, but where there is absorption either by conductivity or molecular resonance $\mu^2 = K + bN$, the second term consisting in the latter case of a series of terms each containing N the number of electrons in unit volume, and corresponding to frequencies at which resonance occurs The mobility of the electrolytic ions has here no influence for the resonance considered is electronic not ionic.

We may then write $\mu^2 = K + b/\rho$, or $K\rho = \mu^2\rho - b$.

It has been shown[1] that the conductivity of saline solutions is nearly proportional to the percentage g of added salt, so that $g\rho = a$, a constant; and further that the change of refractive index is also proportional to the added salt.[2]

Thus $(\mu - \mu_0)/g = c$, a constant, and $\rho = ac/(\mu - \mu_0)$, so that

$$K\rho = \frac{ac\mu^2}{\mu - \mu_0} - b.$$

For saline solutions the values of $\mu^2/(\mu - \mu_0)$ are as follows[3]:

Added Salt per Cent	ρ	$\mu^2/(\mu - \mu_0)$
1	65	1010
5	14·9	204
10	8·26	103
20	5·1	46
30	4·63	36

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[1] Whetham. Theory of Solution. p. 413.

[2] Schütt. Landolt and Bornstein Tabellen. p. 684.

[3] Roy. Soc. Proc. B., vol. 85, 1912. p. 332.

The product $K\rho$ therefore decreases as the strength of solution is increased and since the curve of change of ρ resembles closely that of $\mu^2/(\mu - \mu_0)$ the change of the true dielectric constant K must in this case be small It is evident from this that the resistivity of a conducting dielectric is the dominating factor in its behaviour in alternating fields even at high frequencies and much more so at low values.

Condenser with ionised medium.—The theory of a condenser containing an ionised dielectric medium has been given by Prof. H. A Wilson in connection with the conductivity of flames for rapidly alternating currents. The conditions in flame are in some respects not unlike those of conducting liquids. Their resistivity is about 10^5 ohms per cm. cube. Both flame and electrolytic ions obey Ohm's law. Faraday's laws of electrolysis apply also to salts in the state of vapour, and the charge carried per unit mass is 96,440 coulombs in electrolytes, 98,600 in vapours at 1,400° C. On the other hand the negative ions in flame have a velocity of 10,000 cm. a second, and are probably free electrons; the positive ions of any alkali salt move at 60 cm a second. Neglecting the inertia of the negative ions and their viscous damping and considering the positive ions to be too massive to move, the expression derived for the change of apparent capacity per unit area caused by conductance of the medium is $\sqrt{\frac{ne}{8\pi V_0}}$, where n is the number of ions per per sq. cm. and e the charge on each;[4] V_0 is the maximum voltage during the cycle. The apparent capacity is therefore *independent of frequency.*

In the case of electrolytes the velocity of translation is much less than in flame, but the concentration is so much greater that the apparent dielectric constant will, when measured, be probably found to be higher in conducting liquids than in their vapours at high temperatures

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[4] The Electrical Properties of Flames and Incandescent Solids. Prof. H. A. Wilson, F.R.S Univ. of Lond. Press, 1912. p. 105.

The facts may be summarised as follows The dielectric coefficient of a conducting dielectric, found by observation of the charge in a condenser having it as medium in contact with metallic plates, is proportional to the product $K\rho$, the current density being constant The dielectric coefficient of such a medium should be theoretically independent of frequency; the observed coefficient for water, measured with the liquid in contact with the terminal plates, is independent of frequency. The product $K\rho$ in saline solutions decreases nearly in proportion to the resistivity, and lastly, when the effects of conduction currents are eliminated the observed dielectric coefficient is much reduced, approaching that of a perfect dielectric with $K = \mu^2$.

It is now suggested that the high values of K, for water and alcohol for example, should be considered as an effect depending more on the electrical conductivity of the medium than upon true polarisation, that is to say, more upon the *rapidity* with which the electrical configuration in the molecule can take up new positions than upon the *magnitude* of the change. The conductivity considered is that of electronic mobility within the molecule rather than of ionic mobility through the fluid The resistivities of a number of solid dielectrics have been determined by the movement of charge in a suspended ellipsoid,[5] and agree closely with those obtained by other methods, so that, even in solids, resistivity to polarisation is an important factor.

In conclusion it may be remarked that in the Clausius-Mossotti formula $\frac{K-1}{K+2} = \beta d$, the density d might well be replaced by the conductivity σ, for the general expression is $\frac{K-1}{K+2} = \frac{4\pi N e^2}{3f'}$, in which N is the number of electrons in unit volume each carrying a charge e, f' being the intensity of the elastic constraint caused by the other electrons in the same molecule. The expression might with equal truth be written $\frac{K-1}{K+2} = \beta\sigma$, provided that there is no change of physical state. There are at present no experimental values of both K and σ recorded for any substance at different densities. In the case of conducting dielectrics it will be necessary to measure the coefficient K by a suspended ellipsoid or slab method, by which ionic conduction currents are prevented from flowing

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[5] Proc Phys. Soc. Lond., vol. xxii; also Phil. Mag. March, 1910, p. 403.

THE TIME-AVERAGE VALUE OF URANIUM AND ITS CONNECTION WITH GEOLOGICAL TIME MEASUREMENTS.

By ROBERT W LAWSON, B.Sc.

[Read November 21st, 1912.]

That lead is the ultimate product of disintegration of uranium is now fairly well established. It follows that, provided we know the rate of production of lead per annum from one gram of uranium, and the amount of lead and uranium now present in a primary uranium bearing mineral, it is possible from these data to find the age of the mineral, and thus of the igneous rock from which it has crystallised. This method of determining the age of minerals was used by Holmes[1] in 1911 for minerals of Devonian age, and results for older minerals were given from data provided by Boltwood.[2] The expression used by Holmes in the age determination is $\frac{Pb}{U} \times 8{,}200 \times 10^6$, this giving the age in years.

The decay of uranium follows an exponential law (see Graph I), so that the rate of disintegration at any time is proportional to the amount of uranium present at that instant. Now during the time succeeding the formation of a uranium mineral, the quantity of uranium present, and hence also the rate of production of lead continuously decreases. Consequently, knowing the quantity of accumulated lead, we cannot use the initial or present rates of production of lead per annum in determining the age of the mineral, as these are respectively too large and too small, and result in deficit or excess ages. It therefore becomes necessary to use a mean value of uranium content corresponding to that rate of disintegration which, if continued uniformly throughout the period concerned, would give the existing quantity of lead. This mean quantity of uranium is called the "Time-average Value," and may be represented by U_m.

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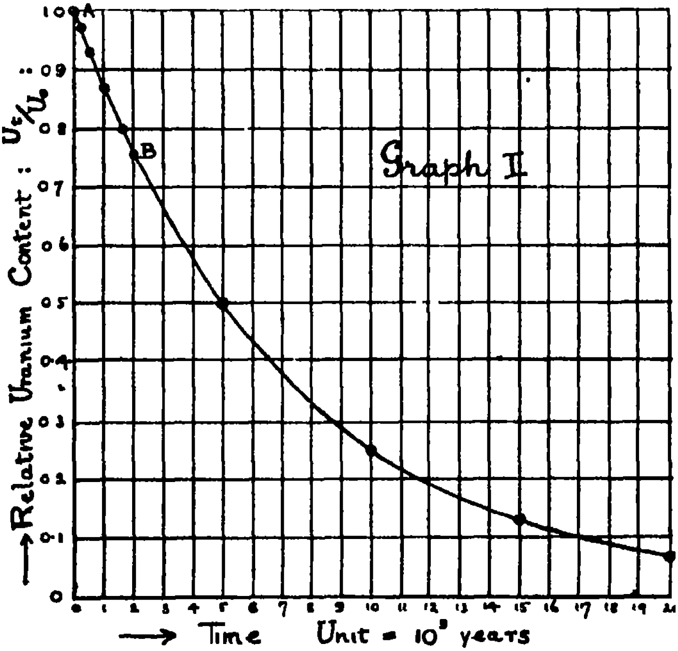
[1] Holmes, *Proc. Roy. Soc. A.*, 1911, vol. 85, p. 248

[2] Boltwood, *Am. Jour. Sc.*, 1907, p. 77. See also A. Holmes, "*The Age of the Earth*," London, 1913, pp. 157-162.

In his calculations, Holmes used the arithmetic mean between the initial and present amounts as an approximation for the time-average value of uranium in the mineral. It seemed worth while to investigate the error so involved, and the present work was undertaken with this end in view.



A mathematical expression for the true time-average of uranium can be obtained as follows, where

U_0 = original uranium content.

U_t = amount of uranium remaining after t years.

λ = disintegration constant of uranium.

The amount of uranium disintegrated in t years $\Big\} = U_0 - U_t$ . . . (i.)

Also, using the value of the rate of disintegration corresponding to the time-average value of uranium, we have

Total Disintegration in t years

$$= -\left(\frac{dU}{dt}\right)_m \times t$$

$$= \lambda . U_m . t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{ii.})$$

Now (i.) and (ii.) are equal, whence

$$U_o - U_t = \lambda \, U_m \, t$$

Or, Time-Average Value of Uranium $(U_m) = \frac{U_o - U_t}{\lambda t}$.

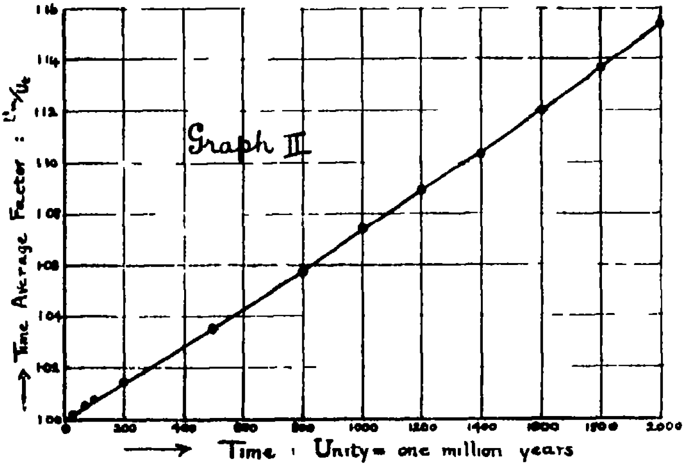
Calculations based on this formula indicate that in general the time average value of uranium is less than the arithmetic mean value, $\frac{U_o + U_t}{2}$. The difference, however, is quite inappreciable except over extremely long periods of time, and from the table of results which follows, it will be seen that the error increases fairly regularly with the time.

TABLE OF RESULTS.

I T in 10^6 years.	II U_t/U_o	III U_m/U_o	IV $U_{A\,M}$ U_o	V Factor $\frac{U_m}{U_t}$
0	1·000	1·000	1·000	1·000
30	0·996	0·998	0·998	1·002
70	0·990	0·995	0·995	1·005
100	0·986	0·993	0·993	1·007
200	0·972	0·985	0·986	1·014
500	0·933	0·965	0·966	1·035
800	0 895	0·946	0·947	1·057
1000	0·870	0·934	0·935	1·074
1200	0·847	0·922	0·923	1·069
1400	0·824	0·909	0·912	1·103
1600	0·801	0·897	0·900	1·120
1800	0·779	0 885	0·889	1·136
2000	0·758	0·874	0·879	1·153

It will be noticed that for a period of 2,000 million years the error involved by using the arithmetic mean content of uranium in the mineral barely exceeds one-half per cent. This introduces a corresponding error of one-half per cent. in the age of minerals of the order 2,000 million years. A further indication that Holmes' assumption is justified is to be found in the fact that the part AB of Graph I.[3] is practically a straight line Thus the rate of decay of uranium alters very little during so long a period as two thousand million years

In the above table, Column II. represents the proportion of original uranium remaining after a time *t* million years. Columns III. and IV. represent respectively the relative time-average and arithmetic mean values of uranium for the times indicated. The last column gives the time-average factor, *i.e.*, that factor by which the present uranium content of the mineral must be multiplied in order to obtain the time-average value. Graph II. has been obtained by plotting this factor against time.



We may now consider three possible methods of applying the results hitherto obtained in the determination of the age of minerals. The first of these is the method used by Holmes, and possesses the advantage of quick application.

(1) The disintegrated uranium is regarded as going to

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[3] Graph I. has been plotted from the relation $U_t = U_o \,.\, e^{-\lambda t}$. The half period value of uranium, *i.e.*, the time required for half the original uranium to disintegrate, is 5,000 million years, whence the constant λ can readily be shown to be $\frac{69315}{5 \times 10^9}$ grams per gram per annum.

form helium and lead according to the relation

$$\underset{238}{U} \rightarrow \underset{32}{8\,He} + \underset{206\cdot5}{Pb} \ .$$

Whence we have

$$U_o - U_t = 8\,He + Pb.$$
$$= 1\cdot15\,Pb \text{ (numerically).}$$

This is the expression used by Holmes in the publication cited, and from which he readily derives

$$\frac{U_o + U_t}{2} = U_t + \cdot575\,Pb$$
$$= \text{arithmetic mean value of uranium.}$$

From the result so obtained the age of the mineral can be found by direct substitution in $\frac{Pb}{U} \times 8{,}200 \times 10^6$, or, assuming $U_{am} = U_m$, by first finding the value of $\frac{U_m}{U_t}$ for the mineral, and then referring to Graph II

(2) Otherwise, an idea as to the correct order of magnitude of the age would first of all be obtained, using the present uranium content By reference to Graph II. the factor corresponding to this age could be obtained, and hence the time-average value of uranium for the mineral, which would be used to give the more accurate time measurement.

(3) Obviously, the most accurate procedure would be to use the arithmetic mean value of uranium to give the approximate age of the mineral; then from the graph obtain the factor $\frac{U_m}{U_t}$ corresponding to this age, this in turn leading to the more correct time estimate. For minerals of age about 400 million years, these last two methods give the same result, whilst for minerals whose age is about 2,000 million years, the error involved by use of the former method amounts to less than one per cent

Since the present paper deals with what is found to be a small correction in the radio-active method of determining the age of minerals, perhaps a brief consideration of other sources of error is justifiable at this juncture.

It is held by many workers in the field of radio-activity that most of the lead dealt with in the present method is 'original' and not produced lead. Undoubtedly lead enters into the composition of many of the secondary uranium minerals and it is owing to this fact that careful judgment must be exercised in the selection of minerals wherewith to determine ages. The minerals must be fresh stable and primary minerals unlikely to contain much original lead. The lead caught up in the molecular network of a crystal during its growth would probably vary greatly from mineral to mineral and hence we would expect the $\frac{Pb}{U}$ ratio for different minerals from the same magma to differ. That no appreciable variation exists was clearly shown by Holmes (*l c c t*) in his analyses of Brevig minerals from the same igneous intrusion. We can but conclude that at least in this case the original lead is insignificant compared with the produced lead. Moreover the excessively high ages given by many minerals is probably due not so much to the original lead present as to the fact that in the weathering of a mineral lead would less readily be removed than uranium this leading to excessive ratios of lead to uranium. Such minerals would give ratios out of keeping with the recognised scale of time and immediately arouse suspicion as to their suitability for age determinations.

Owing to the probability that the whole of uranium is not transformed along the uranium lead series it would appear that the actual quantity of uranium[4] estimated is

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In determinations of uranium by the solution method which was used by Holmes it has been mentioned by Joly (*Phil Mag* 1912 xix p 695) that owing to adsorption or precipitation the estimated amount of uranium would be expected to be too small. This point is emphasised by comparison of the results for rocks of the earth's crust by the fusion and solution methods of determination of uranium. Eve and McIntosh (*Phil Mag* 1907 xiv p 237 *Proc Roy Soc Canada* 1910 3rd series p 67) have investigated the effect of foreign substances on the amount of emanation obtained from a radium solution, and their results together with those of Strutt seem to indicate that at least in the case of the minerals used by Holmes error due to this cause must be very small when the melt is dissolved in hydrochloric acid. In the event of any suspended particles or foreign matter in the emanation flask it is fairly certain that contamination of these with the radium present in solution would result in partial occlusion of the emanation and a resultant deficiency in the estimated quantity of uranium. In the case of minerals of relatively large uranium content it would thus seem advisable to check the determinations of uranium by a purely chemical method. This procedure would immediately expose any such deficiency too large for the age calculations. It seems certain that the actinium series exists as a branch series, and Rutherford[5] has calculated that approximately eight per cent. of the uranium is thus used up. Unless the end product of the actinium series is also lead, it will be necessary to await accurate determination of the above proportion before correction can be applied to the uranium estimate. That any other appreciable sub-series exists is hardly likely owing to the small range the alpha particles of any such series must have in order to have so far escaped detection. Be it mentioned that the effect of the presence of sub-series in the main disintegration series of uranium is to make the age of minerals as determined by the radio-active method too small.

Finally, since the oldest geological formations probably do not exceed 2,000 millions of years, it is sufficient to use the arithmetic mean content of uranium of a mineral instead of the true time-average value, the error involved for this age amounting only to one-half per cent Moreover, correction of this error tends to increase the age estimate. Referring to the other sources of error in respect to the lead and uranium values previously discussed, it would appear that the probability of the actinium sub-series will involve by far the greatest correction. The nett result of these corrections then, is to make the present radio-active age determinations minima On the other hand, Professor Rutherford[6] has suggested that they will be maxima, owing to the possibility of deposition of some lead during the formation of the mineral. Estimation of this last quantity is not possible, as it is most likely widely variable with different minerals. From previous considerations, however, for primary uranium minerals it would hardly seem probable that in general this error would annul the counter error due to sub-series in the uranium disintegration series.

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[5] Rutherford, *Radioactive Substances and their Radiations*, Cambridge, 1913, p. 523

[6] Rutherford, *ibid.*, p. 598.

IONIZATION IN GASEOUS MIXTURES BY RÖNTGEN RADIATION.

By LEWIS SIMONS, B.Sc., (Lond.).

[Read December 9th, 1912.]

INTRODUCTION.

The process by which a gas is rendered conducting during the passage of Rontgen Rays has been investigated by many experiments during recent years; the investigations have thrown much light upon problems relating to the constitution of matter.

For this work the direct heterogeneous primary beam of X-rays emitted from the anti-cathode of an X-ray tube cannot be employed, but this beam is allowed to fall upon a plate of some element which then usually emits both a scattered heterogeneous radiation and one or more radiations homogeneous in character. In the case of carbon, for example, the scattered radiation completely masks the homogeneous beam if this exists, whilst in the case of silver the scattered radiation can be cut off by passing the secondary beam through a sheet of aluminium 5 mm thick. The secondary radiator will not emit its characteristic radiation until the primary beam contains radiation harder, *i.e.*, of shorter pulse thickness, than the homogeneous secondary radiation[1] emitted by it. It has been shown[2] that under certain conditions when this secondary homogeneous beam traverses a gas, an element of the gas itself can be made to emit a fluorescent radiation characteristic of that element. The same remarks apply also to this tertiary radiation, viz., that the exciting secondary radiation must be harder, by Stokes's Law, than the excited tertiary radiation. The emission of this radiation by one of the gaseous elements is accompanied by an increase in the conductivity or ionization and a consequent increase in the absorption of the secondary radiation exciting it.

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[1] In recent work this has been called the primary beam.

[2] Barkla, *Nature*, 1909, Lxxx, p. 187.

All bodies when exposed to Röntgen rays emit a secondary radiation independently of the physical conditions of the body: that the gaseous atom of bromine or iodine emits the same definite spectral line of X-rays as the solid atom, has been shown by Chapman,[3] his secondary radiator being a box with an aluminium window, containing the vapours of ethyl bromide or methyl iodide. The penetrating power of the secondary radiation from these vapours was found to be identically the same as that from the solid compounds of

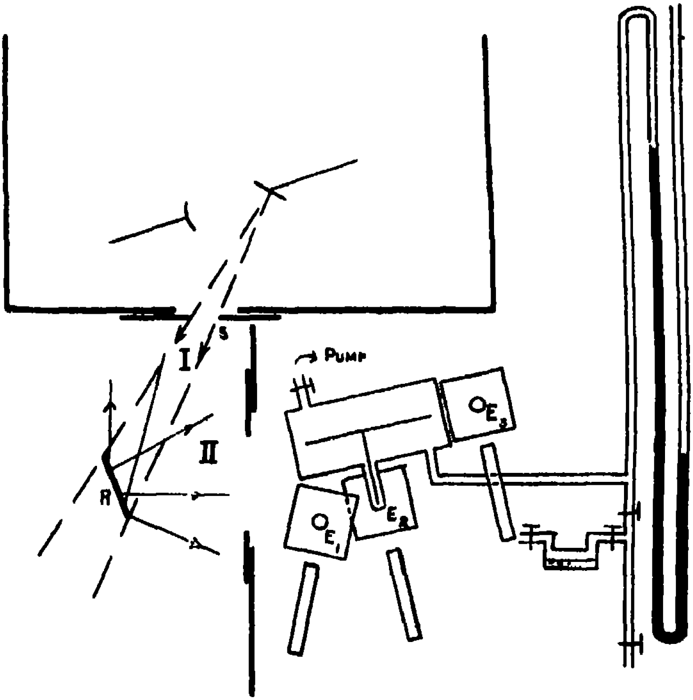

FIG. 1.

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[3] *Phil Mag.*, 1911, xxi., p. 446.

bromine or iodine. It is during the emission of this characteristic radiation in the vapour itself that the ionization relative to some simple gas, air for example, shows such a marked rise. The fluorescent radiation is thought to produce what has been called an electronic radiation.

DESCRIPTION OF APPARATUS.

Fig. i. shows the general arrangement of the apparatus. The X-ray tube itself is surrounded on five sides by a box covered with lead 2·5 mm. thick, and the primary beam is allowed to pass out from the front of the box through an adjustable lead slit S. The secondary radiator R is put in the path of the beam. Any instrument placed behind another lead screen as shown, will receive none of the primary radiation, but only the secondary radiation from the plate R and from the air included by the beam of primary rays.

The ionization chamber consists usually of a cylindrical box—in actual experiments chambers 1, 11 5, and 13 cms. in length were employed which is closed at the incidence end by a thin aluminium window, the other end being covered with aluminium plate or paraffin wax Now the incidence of X-rays upon metals causes them to emit corpuscles, comparable in every way to the photo-electric effect, and since it is necessary to simplify the already complex phenomena in the ionization chamber, this is lined with aluminium foil which, being of a low atomic weight, emits but a small corpuscular radiation under the influence of X-rays The aluminium wire electrode inside the ionization chamber is connected to the leaf of a simple gold-leaf electroscope of the Wilson type. E_1 is a second electroscope, the incidence face of which is covered with very thin aluminium foil. E_3 is a third electroscope which can be employed for finding the absorption coefficient of the secondary radiation in the gas or vapour contained in the ionization chamber which must then be adapted to this special purpose. The movements of the gold leaves are observed by low power microscopes containing eye-piece micrometer scales. The voltage of the leaves is not allowed to fall more than a very small fraction of the total voltage to which they are charged; thus it is correct to assume that the ratio of the deflections of E_2 to E_1 gives a measure of the intensity of the ionization in the ionization chamber, since the conditions of the electroscope E_1 are not altered throughout the experiments. This standardizing electroscope E_1 only becomes necessary because of the impossibility of obtaining a steady source of X-rays and consequently an equally steady source of secondary rays. Finally to the inlet tube of the ionization chamber is connected a mercury manometer and the necessary apparatus for making or supplying the gas or vapour to be experimented upon.[4]

It should be noticed in passing that the term "ionization" does not necessarily mean the ionization as it is observed in the apparatus, but the effect obtained in a layer of gas 1 cm in depth exposed to a radiation of uniform intensity throughout. Shallow layers of gas will actually be exposed to a more intense radiation than deeper ones.

The relative ionization of a gas in a chamber of length l is given by $\frac{i_g}{i_a}$ in the expression

$$\frac{I_g}{I_a} = \frac{i_g\left[\frac{1-e^{-\lambda_g l}}{\lambda_g}\right]}{i_a\left[\frac{1-e^{-\lambda_a l}}{\lambda_a}\right]}$$

in which $\frac{I_g}{I_a}$ is the observed relative ionization, λ_g and λ_a are the absorption coefficients of the X-rays in the gas and air respectively. This correction is quite appreciable except where very short ionization chambers of the order 1 cm. in length are employed or where the absorption coefficient in the gas is extremely small. The magnitude of the correction is shown by the following figures (Table I.).

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[4] It will be seen that a new method of investigation has been introduced. In the work previously attempted the vapours were never experimented upon when pure, but when diluted down to atmospheric pressure with some other gas.

TABLE I.—IONIZING RADIATION. THE FLUORESCENT RADIATION FROM COPPER.

Gas Ionised	Length of Chamber	Absorption Coeff	Observed Rel Ionization	Rel Ionization Corrected for Absorption
SO_2	11·5 cms.	·134	6·0	11·1
Air	11·5 cms.	·0109	—	

In a recent paper by Professor Barkla and myself[5] results are given of ionization experiments with the gases, air, H_2, O_2, CO_2, H_2S, SO_2, and with the vapours of C_2H_5Br and CH_3I. In this paper some further experiments are described on mixtures of some of these. Various penetrating powers of the exciting secondary beam were employed in order to find the relationship between cathode and X-ray ionization when the gas is excited by X-rays harder than the characteristic radiation from one of the gaseous elements. Estimates given by various experimenters of the ionization due to corpuscular radiation range from none to even all of this total ionization. The difficulty arises out of the impossibility of separating the direct, from the cathode ray, or indirect, effect.

PROBLEMS IN IONIZATION.

1. The absorption of X-rays has always been found to be atomic, *i.e.*, the same number of atoms, no matter how they may be combined, will absorb X-rays to the same extent. The absorption of X-rays represents an absorption of energy. Ionization and the emission of a characteristic and a scattered radiation are the only manifestations we are aware of at present of that energy. What, then, is the relationship between ionization and absorption; more particularly is ionization purely atomic?

2. By what mechanism is the ionization and absorption

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[5] *Phil. Mag.*, 1912, xxiii, p. 317.

increased when an element of the gas ionized begins to emit its own characteristic radiation?

3. If this radiation produces cathode particles, how much of the total ionization is due to these?

EXPERIMENTAL RESULTS

To the first question a perfectly definite answer can be given. Ionization is not an atomic phenomenon. The ionization in a mixture of SO_2 and H_2 in equal parts was compared with that in a mixture of H_2S and O_2 in equal parts. In these mixtures we have the same number of atoms of O_2, H_2 and S, but differently combined. The absorption of X-rays in the two mixtures will therefore be equal. The ionizations produced were not equal.

TABLE II. IONIZING RADIATION · THE FLUORESCENT RADIATION FROM SILVER.

Length of Ionization Chamb	Gas Ionized	Corrected Ionization Relative to Air	Absorption Coeff at 76 cms Press
11·5 cms.	Air	1	00077
	H_2S	15·6	·0060
	SO_2	12·9	·0079
	$H_2S + O_2$	8·65 } Ratio = 1·17	·0039
	$SO_2 + H_2$	7·4 }	·0039

It will be noticed from Table II.[6] that the pure gases H_2S nd SO_2 are ionized in the reverse order of their absorption coefficients, and even in the last two experiments in which we secured equal absorption coefficients, the ionizations were still unequal. The questionable atom in this case is the sulphur, for under the stress of silver X-rays it must be radiating, let us suppose, a fluorescent and a corpuscular radiation. From results described later it appears that this corpuscular radiation is responsible for an equal number of ions in both gases if it be totally absorbed in the surrounding gas. The fluorescent radiation from the sulphur would probably produce a greater ionization in the oxygen than in the hydrogen, so that to account for the greater ionization in H_2S we are left with the possibility that the fluorescent radiation from the H_2 might ionize S more than that from O_2. If all the radiations from H_2, O_2 and S be absorbed equally in the mixtures of H_2S and O_2, SO_2 and H_2, equal ionizations should be produced, but the greater ionization in the former mixture may be due to the fact that the soft fluorescent radiation from the H_2 might ionize its combined S atom more than when the S atom is combined with O_2. Apart from these considerations we are bound to say that ionization is not atomic.

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[6] Barkla and Simons, *loc. cit.*

IONIZATION IN ETHYL BROMIDE VAPOUR.

The vapour of ethyl bromide at its saturation pressure at 0°C, which is 16·6 cms. of mercury, is ionized by copper X-rays about eight times as much as air at a pressure of 76 cms. of mercury, and by silver X-rays more than forty times as much as air. Mixtures of ethyl bromide and other gases were therefore ionized with silver X-rays, in order to see whether a relatively large number of molecules of a less active gas could materially affect the radiation from the bromine atom. Very little effect in this direction could be detected. In these experiments the ionization chamber was exhausted by means of a filter pump, and whilst the pump was still running, connection was made by opening a tap between the chamber and a small vessel containing a quantity of pure liquid C_2H_5Br Thus the whole of the residual gas could be swept out of the chamber, which could then be filled to any desired pressure with the vapour up to the saturation pressure at the temperature of the room. The ionization of the pure vapour was first determined, after which a quantity of another gas was allowed to pass into the chamber and the ionization redetermined. In no case was any striking anomalous effect observed; in every case the ionization of the ethyl bromide when diluted was nearly the same as when pure

TABLE III.—IONIZING RADIATION. THE FLUORESCENT RADIATION FROM SILVER

Length of Chamber	Mixture Ionized		Rel. Ionization Corrected for Absorption	Ionization of Gas	Ionization due to C_2H_5Br
	C_2H_5Br	Gas			
13 cms.	14 4 +	0	38·1	0	38 1
	,,	$+ H_2$ 48 8 cms.	36 2	0·9	35·3
	,,	$+ SO_2$ 48 8 ,,	42·8	7·4	35·4
	,,	$+ H_2S$ 46·3 ,,	46·0	9·8	36·2

It is apparent from the figures in column 5 that the ionization in ethyl bromide alone is always slightly greater than when the vapour is mixed with another gas, after the ionization due to the gas has been subtracted from the total effect. The magnitude of this fall is not very marked for two such different gases as hydrogen and sulphur dioxide. These results could be accounted for by two theories: (1) that there is no corpuscular radiation from the bromine at all, (2) that if there be a cathode radiation from the bromine, its ionizing effect is the same in H_2, SO_2, H_2S and even in C_2H_5Br itself for complete absorption; and it would be completely absorbed in the dimensions of the gas chamber, except at the margins. Although the actual presence of a cathode radiation has not yet been shown experimentally, the first theory is not so tenable as the second, since we do know that the bromine is emitting X-rays, which has always been associated with the acceleration or retardation of a corpuscle.

The above results suggested a further series of experiments in which varying quantities of the gases were mixed with a standard quantity of ethyl bromide vapour. The results are shown in Table IV.

TABLE IV.—LENGTH OF IONIZATION CHAMBER = 13 CMS. ALUMINIUM WINDOW AND END. SILVER RADIATION

Partial Press. of C_2H_5Br and H_2, Air or SO_2	Observed Ionization of C_2H_5Br and			Corrected Ionization of C_2H_5Br and			Ionization due to C_2H_5Br in		
	H_2	Air	SO_2	H_2	Air	SO_2	H_2	Air	SO_2
6·4 + 0	11·4	11·4	11·4	12·2	12·2	12·2	12·2	12·2	12·2
„ + 20·3	10·9	11·3	13·6	11·5	12·2	14·8	11·5	12·0	12·2
„ + 40·8	11·1	11·2	15·7	11·9	12·1	17·2	11·9	11·7	12·1
„ + 61·2	10·8	11·6	18·0	11·5	12·7	19·9	11·5	11·9	12·2

Ionization at 76 cms. pressure of $H_2 = \cdot 04$, Air $= 1\cdot 0$, $SO_2 = 9\cdot 6$.

$$_{Ag}\lambda_{C_2H_5Br+H_2} = \cdot 00142\, p_{C_2H_5Br} + 0 \times p_{H_2}$$
$$_{Ag}\lambda_{C_2H_5Br+Air} = \cdot 00142\, p_{C_2H_5Br} + 0\cdot 00001\, p_{Air}$$
$$_{Ag}\lambda_{C_2H_5Br+SO_2} = \cdot 00142\, p_{C_2H_5Br} + 0\cdot 00014\, p_{SO_2}$$

The numbers in columns 2, 3 and 4 are the ionizations in the mixtures relative to the ionization in air at a pressure of 76 cms. of mercury. Column 3 has been obtained from column 2 by using the absorption coefficients found from the above three expressions deduced from the results of Barkla and Collier;[7] p is the partial pressure in centimetres of mercury. The last column was obtained from column 3 by subtracting the corrected ionization due to the H_2, air and SO_2 respectively. It will be noticed that in the case of hydrogen no quantity has been subtracted; this is because the ionization in hydrogen even at atmospheric pressure is quite inappreciable compared with that in 6·4 cms. of ethyl bromide

From the constancy of the results in the last column, even in cases where there was a greater percentage by weight of SO_2 present than C_2H_5Br vapour, we can say that the cathode radiation from bromine produces the same number of ions in C_2H_5Br, H_2, air and SO_2 and probably in any gas for the complete absorption of its energy: for let us suppose that the ionization in SO_2 by cathode rays is very much greater than in H_2 for complete absorption, a very much greater variation than from 12·2 to 11·5 would have been obtained for the ionization due to 6·4 cms. of C_2H_5Br together with, in the first case, 61·2 cms of SO_2, and in the second case, 61·2 cms. of H_2. In the former case we have by far the greater part of the cathode rays absorbed in SO_2, whilst in the latter it is practically all absorbed in the C_2H_5Br.

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[7] *Phil. Mag.*, 1912, xxiii, p. 997.

IONIZATION BY CATHODE RAYS.

We[8] made a flat box ionization chamber 8 cms. square and 1 cm. deep. The faces were of cut carbon plates 1·5 mm. in thickness which, in one part of the experiment, were covered on the inside with ordinary gold leaf. The ionization produced in the contained gas when the interior faces were of carbon was due to the X-radiation chiefly, but when of gold to an intense cathode radiation emitted by it when subjected to a beam of silver X-rays.

TABLE V.—CORPUSCULAR RAY IONIZATION.

Gas	Observed Ionization Carbon Ends	Observed Ionization Gold Ends	Ionization due to Corpuscular Radiation	
$H_2S + O_2$	6·38	15·7	9·32	Ratio = 1·20
$SO_2 + H_2$	5·74	13·5	7·76	

If the cathode radiation from the gold was absorbed equally in the two mixtures, then the ionization produced was practicaly the same as that produced by any type of X-rays. On the other hand it should be pointed out that Kleeman[9] has obtained results for the relative total ionization produced when cathode rays from gold are completely absorbed in widely differing gases, these relative total ionizations differ by a small amount.

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[8] Barkla and Simons, *loc. cit.*

[9] *Proc. Roy. Soc. A.*, 1910, vol. lxxxiv, p. 21.

Owen, too, has shown[10] that the total number of ions produced by homogeneous beams of X-rays of equal intensity is the same in the gases CO_2 and SO_2 for any particular type of rays. Exactly the same remarks apply to the ionization by the α particle We see therefore that cathode rays possess many properties similar to those of X-rays.

If we assume that all the ionization in C_2H_5Br is direct, we should have to suppose that 40 molecules are ionized during the passage of silver X-rays relative to air, to 8 during the passage of copper X-rays.

If the total absorption of cathode rays produces the same number of ions in any gas, *i e*, if the total ionization due to a corpuscle is proportional to the number of collisions with surrounding molecules and not dependent on the nature of the molecule struck, we arrive at a satisfactory expression for the ionization produced in a mixture under any conditions.

The ionization in a mixture of C_2H_5Br and another gas when no fluorescent X-ray spectral line is emitted by any element of it is proportional to

$$k_1\omega_1\mu_1 + k_2\omega_2\mu_2$$

in which μ_1 and μ_2 are the ionizations relative to air for the C_2H_5Br and gas respectively, for the same expenditure of X-ray energy, k_1 and k_2, are the mass absorption coefficients, ω_1 and ω_2 are the masses of C_2H_5Br and gas per c c.

The ionization in a mixture when cathode ray ionization is present is

$$k_1\omega_1\mu_1 + k_2\omega_2\mu_2 + nk_1\omega_1$$

since the number of cathode particles produced will be proportional to $k_1\omega_1$ and one cathode particle produces n ions in any gas. This expression is strictly additive and will account for the results in Tables II. and IV. In Table IV., column 4 is represented by this expression without the $k_2\omega_2\mu_2$ term, it is therefore constant if ω_1 is constant.

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[10] *Proc. Roy. Soc. A.*, 1912, vol. lxxxvi, p. 438.

SUMMARY OF RESULTS.

In this paper some further experiments are described on ionization in gaseous mixtures under a new variety of conditions.

Ionization by X-rays is not an atomic phenomenon such as is the absorption of X-rays.

If a large amount of the ionization in a mixture of ethyl bromide and another gas is due to a corpuscular radiation from the bromine, the ionization produced by the total absorption of these corpuscles is the same in any gas.

Corpuscular rays or cathode rays possess many properties similar to those possessed by X-rays.

My heartiest thanks are due to Professor Stroud who has afforded me every facility for carrying out this research.

ON THE SYNOPTIC ASPECT OF REALITY.

By J. THEODORE MERZ, Ph.D., D C L., LL.D.

[Read February 6th, 1913.]

In a paper which I read some time ago before this Society I dwelt upon a general tendency of philosophical thought during the latter part of the nineteenth century. In the third volume of the "History of European Thought" I have, in several passages, referred to this tendency of thought and I shall have still fuller opportunities to do so in the fourth volume which is now in preparation.

In writing the History of Thought during a certain period it seemed to me essential to settle in my mind two points:

First. Before a review of the whole subject of Philosophical Thought could be attempted it seemed important to fix, as it were, the position in time and thought from which a survey could be made.

Second. At the end of the survey or the narrative it would be desirable to bring out, if possible, some definite aspect or aspects on which the various courses of thought appeared to be converging.

It is inevitable that the fixing of these two definite points would be, to a large extent, subjective; for the writing of history is much more of an art than of a science. However, as in every other art, a large amount of science in the form of method, knowledge or erudition is wanted, so also in the writing of history a large amount of scientific preparatory work is required; and yet such, be it ever so complete and thorough, would remain unsatisfactory if the artist or writer did not choose a definite position from which to survey the whole of his object or did not bring into focus the image which it had produced on his mind.

So far as the first point is concerned, the selection of a position from which to make my survey I have, for reasons which to some extent are personal, chosen the middle of the century as a convenient position from which to look backward and forward, and I have identified this position to a large extent, though not altogether with the name of Lotze. I have indicated this in the early part of the third volume and followed it up in subsequent chapters.

The second point should naturally be brought out with greater clearness at the end of the forthcoming volume which should close the History of Philosophical Thought. Inasmuch, however, as I have had to depart from my original intention of publishing the two volumes together, I have felt the necessity of anticipating, in the interest of my readers and critics, something of what really belongs *to the closing chapter of the second section, i.e.,* vol. 4. I may here state at once that, when answering the question as to the general outcome or tendency of the philosophical thought of the century, I shall have, in many points, to leave behind me the position taken up in the course of the exposition and show that we seem to have more recently got beyond the position and the formulas of Lotze.

In this paper then I do not propose to identify myself with the Lotzian aspect in the same degree as I have thought it advisable to do in the History, but rather to bring out that tendency which is more clearly revealed only in the latest speculations, and this perhaps most prominently in this country, though the first indications of it are to be found independently in the philosophical literature of the three countries in which I am more immediately interested.

In the paper I refer to I desired to elicit the opinion of my hearers and readers as to a definite term under which to designate the tendency I had in view. Immediately after writing it I came across a term in an early tract of Auguste Comte's which, though I had, both there and in some of Mill's reviews, read it before, had at the time not impressed me with its value and importance. The term used is the *ensemble* of things and Comte distinguishes the *vue* *d'ensemble* and the *esprit d'ensemble* from the *vue* and the *esprit de détail*. The German language can translate this as: *Das Beisammen*, the literal translation into English is what I used in the paper referred to, the "Together" of things; however, my friend, Prof. Sorley of Cambridge, has in correspondence suggested to translate the *vue* or *esprit d'ensemble* by the term "the synoptic aspect," and this I have adopted, as will be seen in various passages of the volume just published

The aspect which I want to bring out, and which we will call the synoptic aspect, has two sides in the same way as the words "view," "sight," "sensation," "perception," and their synonyms in French and German have an objective and a subjective side with an equal or an unequal emphasis on the two sides. We may speak of a "view" or a "sensation" meaning a definite thing or object and we may use the same terms denoting a subjective state or activity Of all the terms which may be employed in the three languages, combining the two aspects which I refer to, the German word *Anschauung* is probably the most expressive and in its further definition as *Gesammtanschauung* it is probably the best equivalent to the Greek word which I have adopted, viz., Synopsis, and to the French *vue d'ensemble*.

Ruskin has, in a remarkable passage at the close of the third volume of "Modern Painters," used the word Sight in this sense, but his remark has not received the attention nor the word Sight the currency which I think they deserve.

In my former paper I dealt with the *ensemble* or "together" of things in the objective sense. I there maintained what had already been suggested by Goethe on various occasions but notably in a remarkable passage in his well-known treatise "On the Metamorphosis of Plants"; that the things of nature in their natural "together" as they are presented to us contain something and impress our mind in a way which is destroyed by the scientific process of analysis and not regained by the scientific process of synthesis. With this in view I now propose to contrast the process of synopsis with the combined activity of the analytic and synthetic processes.

I also mentioned one reason why scientific or logical synthesis does not succeed in recovering the position from which analysis started. This was the fact that analysis can never be complete and that, in consequence, synthesis always starts, as it were, half-way. As analysis can never reach the end so synthesis can never find and start from the beginning. What this something is which we lose in the process of analysis and cannot regain by the process of synthesis, that something which is given to us in synopsis, is difficult to say—perhaps it is impossible as any attempt to give it expression would have to be carried out in terms of language and this itself originates in a combined analytic and synthetic process The only way of getting at this something is to look at the thing, to contemplate it, to gain a synoptical view, the *Gesammtanschauung*; this, I need not specially observe, is pre-eminently the work of the artist.

But it is not the artist alone who relies more exclusively upon synopsis. Many of the great discoveries in science have originated in quiet observation and contemplation, perhaps even in an artistic rendering of things natural. A friend of mine, a distinguished Professor of Biology in this University, told me only the other day that, when resting during his holidays from the analytic work of his laboratory, he takes to the brush through it to commune with Nature. Now although it is impossible to state in words, or at least in prose writing, what that something is which is revealed to us by Sight and which we cannot fully grasp by the scientific processes of analysis and synthesis we may yet point to certain results which are occasionally gained by naturalists when they give themselves up to the contemplation of, and the communion with Nature, and which must be greatly assisted by the artistic faculty. They see definite things which they did not see before and they see relations of things which did not occur to anyone before them. It will at once be seen that in both these cases Sight has led through attention to clearer definition and to new combination resulting perhaps in a scientific synthesis The former process of clearer definition may be termed a differentiating or analytic process and a great part of the work of science and its advance consists in fortifying the human mind so that it sees more clearly and distinctly. This does not refer only to the fortifying of the physical senses of seeing, hearing, etc., but also to what we term feeling and, in general sensibility, both of which can be made more acute and efficient by education and practice and, in general, by what we call culture.

I now wish to direct attention to the subjective side of our sensations. The commonsense view sees in sensations such as, *e.g.*, the sensation of a colour, two things, the outward existence of the colour and the subjective perception of colour This view, this duplication of something seen and the seeing of something cannot according to a modern school in Psychology—be maintained At the moment of perceiving the perceived and the perceiving are identical. It is somewhat difficult clearly and fully to grasp this truth, but it seems to me to be fundamental and has, I believe, only clearly been brought out in the course of the last fifty years by psychologists such as Mach, Renouvier, James Ward, Shadworth Hodgson and others. Without wishing to identify my own way of expressing this fact with the exact manner in which any other psychologist has stated it, I may merely say that as an historian of thought I have been influenced by the statements, remembered or unremembered, of many other thinkers.

For my present purpose it is unimportant whether we take up the commonsense view implying a duality between the outward thing and the image corresponding to it in the soul of the observer or whether we adopt the modern psychologist's view according to which there is only one original datum, viz., what Ward and others term the "presentation" or "sensory" continuum. Both ways of approaching the psychological problem have their special difficulty. The first or dualistic view, which is adopted in an extreme form in psycho-physics, has to answer the question: How do the external object and the internal sensation correspond? It has, as it were, to bring together, to connect somehow, the two data with which it starts. The other, or monistic view, which in an extreme form is represented by Ernst Mach, but was suggested already but not carried through by Hume, has to answer the question: How does the one original datum, viz., the presentation continuum, come to be separated into the inner world and the outer world.

And here I may remark that, leaving out of consideration the special difficulties, both positions, though perfectly legitimate, commit a mistake if either of them start with the assumption of definite isolated things or definite isolated sensations. Such never exist in reality but are the product of a process of abstraction. In other words, the synoptical aspect, the "together," the *ensemble*, refers as much to what we may term external things as it does to what we may term internal things or sensations. The prius and beginning is always a synopsis, and this has been clearly stated, so far as external things are concerned, by Lotze when he says "to exist means to stand in relation"; and, so far as internal things or sensations are concerned, by James Ward when he points to the presentation or sensory continuum.

Assuming at present the modern psychological view, we have to note that the primitive and original sensory or presentation continuum becomes differentiated in the earliest stage of our conscious mental existence into two continua, the internal continuum, which we call our inner world, and the external continuum, which we call the outer world.

The former is preserved and enlarged through memory; the latter arises and is preserved through a variety of processes which are complex and intricate but among which the two following are probably the most important:

1. Our own unconscious and conscious restlessness or activity entailing repetition, attention, recognition, order, regularity, etc
2. Unconscious and conscious intercourse with others who shew us, and tell us of, this continuum.

Our subjective and objective continua are gained through memory, activity and intersubjective intercourse. We know the outer world largely through what other persons tell or shew us and only to the extent that such an intersubjective communication is possible do we gain in our infancy and, before we are conscious of it, a tolerably distinct and practical view of an external world which external world includes other persons and also ourselves in bodily shape.

We sometimes see it stated that there is no logical way *out* of the ipsistic or solipsistic position Against this I maintain that there is, for the adult mind, no way into or back to such a position, inasmuch as not only the knowledge of an outer world but even that of our own self is acquired and possible only by previous intersubjective intercourse, and that consciousness of self is not possible without that of other selves and of ourself as one among them and among other things The adult person in possession of normal faculties and as such also the philosopher, does not begin to reflect except after a lengthy intercourse with others Of this he, in his infancy, was primarily unconscious; but gradually became more and more conscious. Through it he has acquired two centres for his thought about himself which we may, with William James, Josiah Royce and others term the Individual (subjective) and the Social self. And whether as a thinker he may try to cast one or the other of these two selves into a shadowy distance or temporarily forget either of them, no sustained train of reasoning exists without both of them, or if it exists it carries, as Hume said, no conviction.

Through intersubjective communication we, in fact, gain two views of the sensory continuum, two continua, and these do not coincide with each other. In order to realise the internal continuum maintained and enlarged through memory, with or without present sensations, we have to shut out or forget the external continuum, and in order to describe and comprehend the external continuum, we have to cast aside or forget the internal one. Both processes are extremely difficult and can only partially be carried through The two continua are ever and always being intermixed, they intrude on each other, and the training of science especially is to get rid of the subjective and regard only the objective continuum.

On the other side the demands of practical life necessitate and encourage the continual alternation of the two aspects of things, and we may say that the object of all serious thought during life is to bring the two aspects together and into some kind of relation· similarly as, when looking through a stereoscope we bring two pictures together and are not satisfied till they fall together till we gain a synopsis—giving us one clear view. In the case before us, however, this actual falling together of the two psychical continua never really takes place in our minds, and this gives rise to a continual striving and a more or less marked feeling of mental discomfort. Whereas in the stereoscope we succeed in completely merging two continua into one continuum we only very partially succeed, in actual life and the course of experience, in bringing together the external and internal continua. To do so, we would, as it were, require a new dimension. In other words, the practical continuum of our conscious life includes an inherent discontinuity as also (it may be noted), consciousness itself includes the phenomenon of the unconscious.

But it is not necessary for my present purpose to describe —even if such were possible—more minutely the complicated processes through which consciousness of others and of ourselves gradually emerges out of the chaos and bewildering complexity which must confront the awakening and growing sensibility of the infant mind. I wish to dwell only upon the fact that if what we see and the seeing of it are primarily identical and become duplicated only through memory and intersubjective intercourse that all the properties of what we see (taking Sight in the larger sense of Anschauung) must belong likewise to the seeing mind, the mind being practically merely the conscious continuum of sensations perceived in time and preserved by memory.

There exists then a synoptical view not only of things outside of us but also of our own selves. The totality or 'Together' of inner experience—*die Selbstanschauung*—is more, and something else, than the sum of its differentiated or specially noted and remembered parts or incidents. Language possesses several terms to denote this, but the word 'feeling' which may occur as appropriate, does not denote it for 'feeling' may mean a very definite and isolated sensation. Words like the "state of mind," the 'mood" are more applicable to denote the synoptical aspect of our inner self, and I maintain that such a synopsis is the prius of all conscious life and that this is developed or resolved only by the acquired processes of differentiating of analysis and subsequent synthesis. This initial synopsis is what we term the 'I' or Ego, the unity of the sensory continuum. It cannot be further defined but it is always there as long as consciousness exists and attention to its separate parts or elements is a subsequent and acquired performance.

In a very distinct way we may therefore maintain that all that a closer contemplation of nature, through the fortifying of our senses or the processes of intense thought reveals to us is, as it were, "there" in our minds or in the minds of others who are able to point it out or reveal it to us, to make us see what we did not see before, explaining or interpreting in this way what was unobserved, but nevertheless hidden in our original synopsis. This view is very much the same as that expressed by Leibniz when he said that the "Monad" is a mirror of the Universe.

I will now proceed to give some illustrations of this synoptical aspect shewing how it bears upon sundry practical and philosophical problems I repeat that under this term I wish to bring out the following two points

1 That synopsis everywhere precedes analysis and synthesis and that
2 It reveals or contains more than analysis and synthesis can ever discover or deal with

I stated before that the views I have been trying to expound have been gained under the influence of others whose speculations I have studied and partly assimilated I must now add that they are gained quite as much through practical experience

No fact has been more deeply impressed on my mind through practical experience, e g in matters of business than the great difference which exists among persons as to their capacity of taking the synoptic view of things or, to express it in everyday language, of grasping the affairs they have to do with as a whole regarding always in the light of this synoptic view the details which come before them, referring the latter always back again to the comprehensive aspect which is, as it were, ever present and at the back of their minds This mental attitude is peculiar to, and fully developed in, comparatively few minds and it is they who are the organisers of large enterprises be they industrial, commercial, social or political they are essentially the administrative minds

Not less rare and important for the progress of affairs are, of course, those other minds who have an eminent faculty for concentrating their attention upon single points carrying the processes of abstraction analysis and subsequent synthesis to a high degree of perfection but these are rarely endowed with the synoptic faculty which always sees things in their Together and never anything, however important, in isolation, torn out of its context in the real world be that nature or human society

Those who belong to the former class, those who possess the genius of combined analysis and synthesis, may be the originators, but those who take the comprehensive or synoptical view are the successful organisers and administrators in practical life The working classes who as a rule and especially in recent times are driven to specialisation and likewise many eminent experts in the Arts the Sciences and the Professions do not sufficiently appreciate how indispensable are the organising minds As a rule they appreciate the originating faculty in the discoverer and the inventor inasmuch as these produce something that is novel but they rarely appreciate except sometimes after painful experience the work of the organiser as according to their view he does not produce anything new but only deals with things and knowledge which he receives from outside or from others Their conception of the business problem is analogous to Carlyle s definition of the problem of political economy Given a society consisting of fools and knaves how to produce efficiency and honesty through their combined effort Nothing is more important in the present day when we hear so much about co operation municipal and state enterprise than to impress upon the masses the impossibilty of successfully managing any large affairs without securing and trusting in the work of comprehensive minds who possess the grasp of things who take what I call the synoptic view who have the *esprit d ensemble* the *Gesammtanschauung* who see with their mind s eye the whole before they descend into parts and detail

Coming now to special philosophical problems I see in synopsis the origin of our idea of Self not of what self consists in but of the self or I which is always present with us forming as it were the background of our conscious life It is the first and most important synopsis in our conscious life and is not gained by a synthesis of detached experiences among which *e g* Hume sought for and could not find it it is simply there as a uniting bond Before we feel and experience it and are aware of it we are not able to fix our attention on any detail of sensation or perception This prius of all self conscious life, this internal synopsis does indeed grow or extend in course of life through memory, activity, and intersubjective communion, but it must be there before all these other processes can be of any use or lead to anything. I may incidentally mention that whilst I appreciate and have been much benefited by Mach's "Analysis of Sensations," I feel that there is a want in his analysis: he does not start with the Together of sensations as the prius but considers them only in their isolation, in their analysis and synthesis, which are possible only for the adult mind in a very advanced stage of development.

The second application which I would like to make of synopsis or Sight is that it contains the germ of the ideas of purpose and finality, the relation of a whole to its parts, as opposed to sequence and causality, the relation of the parts to each other. The reason why Mach has no appreciation for the category of finality seems to me to lie in the defect mentioned just now.

A third application I would like to make would be to the ethical problem and this in the direction indicated by Prof. Hoernlé in his criticism of Bergson's Theory of Free Will. To explain this we must not look for a special or isolated intrusion of something into the mechanical order of things, something called "creative energy," but we must look to the totality or *ensemble*, the Together of our whole mental life, given to us by conscious synopsis, by Sight, which in this case does not mean a special intuition but simply a synoptical view.

I will not dwell on the application to the aesthetical problem which is self-evident but proceed to the most important application I would like to see worked out: it is that to the Religious problem. As this must, for any philosopher who is approaching the end of his life, be the most important question, without an attempt towards the solution of which no philosophy, as distinguished from science, is worth much, I must dwell upon it at some greater length.

Let us then first remember the two essential points which the present discussion has tried to establish: the first point is not peculiar to the synoptic view. It is merely a general truth that the source of all our knowledge, thoughts, ideas, convictions, etc., is to be found in the continuum of our sensations, which continuum, however, contains discontinuities and is dependent on, and enlarged through memory and our communion with our fellow men.

The second point which is peculiar to the view I take is this: that the continuum of our sensations cannot be broken up into separate distinct sensations or parts without losing something through:

first, the incompleteness of every such attempt, and,
second, the loss of the uniting bond, the Together under
which sensations are primarily presented to us.

We proceed from Sight to definition and thought, but we can never, through a combination of thoughts and separate parts restore again the Sight or *Anschauung*, the Together or *ensemble* from which we started.

If the individual human mind, developed and enlarged through memory and intersubjective communion, contains always more than the sum total of its isolated experiences and something which lies beyond and underneath them, this undefinable something must have an effect on the whole of our thinking and doing. Let us term it for the moment a sense of the universe or a cosmic emotion. It constitutes synopsis in the largest meaning of the term. We are, however, never satisfied with pure contemplation, the mirror of the Monad, to use Leibniz' expression, is not a merely passive reflector, it contains a principle of unrest or activity. We desire to see more clearly and to understand what we see, to communicate it or show it to others, or to express it in words. We may term this a process of interpretation of our original "presentation" continuum; we desire that daylight be thrown into the half illunminated recesses of our mind.

The first and most important interpretation which is given to us is that which we unconsciously and consciously receive in our own infancy and childhood through the communion with other persons: we will term this the first and most important revelation given to us But as stated before this interpretation or revelation consists in a breaking up and subsequent putting together again of our original "presentation" continuum, it is essentially incomplete and, as it were, artificial There remains a great deal which is never interpreted to us through this revelation and there remains, in consequence, an unsatisfied desire for a further, a larger and more comprehensive interpretation or revelation.

As our early instructors have interpreted and revealed to us a portion and what may be for the ordinary purposes of this life the most important portion -of our innermost possession, so the human mind, in a higher or lower degree, looks for an interpretation or revelation of what, from another point of view, is even more important, an additional interpretation or revelation which would, as it were, complete and join together the incomplete and fragmentary revelation of ordinary life: giving us an interpretation of the Whole of things, an image or a word which expresses our innermost experience This interpretation or revelation is what religion gives us and our belief in any special religious view or doctrine is the feeling that it meets this desire, that it opens our understanding of the totality of things, of the ultimate Reality.

This view of the larger and deeper revelation which the human mind longs for and which is more or less satisfied by the religious or spiritual interpretation of things and by the acceptance of the same, suggests many reflections and recalls well-known expressions and theories put forward by philosophical as well as by religious thinkers. On these I do not wish to dwell at present.

In a chapter of the fourth volume which will deal with the "problem of the Spirit" I shall have an opportunity of enlarging on the various attempts which have been made during the nineteenth century to deal philosophically with the religious problem.

There are only two points which I should like to urge in this rapid and somewhat superficial sketch

I think it is helpful to point to analogies which exist between what I have termed the two great revelations which we receive or may receive during our lifetime. Let us term the first revelation, which takes place unconsciously and consciously during our infancy and childhood, the commonsense view of the World and Reality, and let us term the second and later revelation, which may come to us in the course of our life, the spiritual or religious view of the World and Reality. We may then point to two characteristics common to both revelations:

(1) Neither the earlier nor the later, neither the commonsense nor the spiritual view of things is of much value—the first as a practical work-a-day conception, the latter as a subject of belief and a matter of faith until each has arrived at a sort of stability and completeness We must in both cases be able to settle down to some tolerably compact, coherent and complete view: in other words, and using the terminology of this sketch, the unconscious and conscious experience which we gain must to some extent be expressive of the original synopsis or Together from which we started and which forms the underlying groundwork or material, the primary Together or synopsis of our conscious life. The mind must in both instances arrive at a position which affords some rest and a tolerably firm basis for thought and action, a settled, though it may be never a thoroughly clear and transparent view or comprehension. But only when in possession of such a comprehensive, commonsense or spiritual view of Reality can we descend into details and carry on the work of our life with some amount of satisfaction and success: expressed in other words, both the commonsense view and the religious view must be presented to us as a whole, and single points in either can only be decided and dealt with in the light of the whole. In both cases we must reach a synoptic aspect.

(2) The processes and stages through which we have gained, in our infancy and childhood, that tolerably stable view of Reality which I term the commonsense view, the work-a-day aspect of things, are not known to us in detail. No psychological or biographical analysis will ever reveal to us the infinite number of small experiences, of hidden, unobserved and forgotten steps, through which by an alternation of our own activity and the influence of our instructors we have arrived at that commonsense view of the world which serves us as a solid ground and indestructible conviction during the rest of our lifetime.

Is it likely that, in the case of the spiritual revelation gained by individual effort, the influence of others and the work and experience of many minds in the course of the history of our race, we should be more successful in unravelling the details of growth and formation of what for any individual person has resulted in his or her spiritual conviction or religious faith? In neither case would scientific analysis or historical criticism be in itself sufficient to produce in our minds the conviction of the truth of that aspect of things which has been gained by undiscoverable processes and which stands firm as an article of commonsense or spiritual faith and is intelligible and useful only if viewed in its totality or as a whole. Must we not here also look at the whole and not expect to exhaust or fathom it by scientific analysis or historical criticism of detail?

The unfolding of the individual mind in infancy and childhood remains, as indeed the appearance and history of every new living thing, a mystery and a wonder. Query: Should the appearance and history of the later and larger revelation, worked in many minds during the ages of history, and descending upon us through individual experience, through social environment and influences, contain less of a mystery and a wonder?

I could quote many passages from the works of recent thinkers pointing to, but frequently only vaguely expressive of, the view which I have tried to state more definitely in this *Aperçu*; and in the fourth volume I shall hope to give them in sufficient abundance. Here I will only refer to one of the last deliverances of Wilhelm Dilthey, who left us only a year ago, perhaps the most original thinker whom Germany has of late possessed. In his contribution to a volume with the title "Weltanschauung," published in 1911, he has expressed his conviction that the comprehensive views of Reality (Weltanschauungen) are not the products of thought, and do not arise solely through the desire for knowledge. According to him, they proceed from the experience and tenor of life, from the structure of our mental totality. Thus it is not by a process of co-ordination of a multitude of separate instances and parts, through similarity or uniformity that comprehensive views and convictions are gained, but through a process of synopsis (Zusammenschauung) by seeing the separate parts in a whole, by rising from a comprehensive view gained by living experience to a comprehensive view of the World.

In this his last published deliverance Dilthey has, as it seems to me, given the clearest expression of what, in the long series of previous writings, he has been aiming at. These writings are beginning to attract the attention they deserve not only within wider circles in Germany, but also in other countries, and foremost in France. I have no doubt they will be increasingly appreciated also in this country.

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY

THE GREAT WHINSILL AT KIRKWHELPINGTON

By G. WEYMAN, B.Sc.

[Read January 9th, 1913.]

In a former paper[1] a somewhat detailed account was given of the relation of the Whinsill to the sedimentary beds into which it intruded, as exposed in a section stretching from the North Tyne at Barrasford to Swinburn Mill, and it was natural that an attempt should be made to continue the section north-east along the escarpment. Unfortunately the absence of any exposure showing the contact of whin and sedimentary rocks to any extent made it necessary to pass over for the present the intervening country, and continue the examination some seven miles north-east, where the Wansbeck cutting through the escarpment gives many excellent sections just below the village of Kirkwhelpington.

This district was surveyed by Messrs. Topley and Lebour in 1877, but the Geological Survey maps are too small to show any detail, while the 6" maps have never been published.

The district is also mentioned by Messrs. Topley and Lebour in their paper on the Intrusive Character of the Whinsill,[2] and by Hutton and Tate, the latter of whom state that two sheets of Whin 20 feet and 12 feet thick are found separated by 1 foot of altered shale, but the exact place where this occurred could not be found, and no details are given. It seems probable that this exposure was closer to the junction of the two sheets than those now found.

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[1] *Proc. Univ. Durham Phil. Soc.*, 1911, Vol. 3, pt. 4.

[2] *Q. J. G. Soc.*, 1877.

In the stream bed below the western side of the hill on which Kirkwhelpington stands, and round which the Wansbeck flows, a fault, running nearly due east and west (275°), throws a massive but sharply folded limestone (o) against a false bedded more gently folded sandstone (3). Below the sandstone lie a shale (2) and limestone (1), the latter showing a small anticlinal which brings Whin to the surface a few yards west of the stream-bed. (See map).

Passing down stream one finds the top of the Whin (A) covered by a limestone (4), shale and thin limestone (5), which pass below a second sheet of Whin (B) with its covering of limestone (6). The sandstone (3) is absent while the limestones are evidently different beds from that covering the Whin (A) near the fault. The Survey 1″ maps mark a fault branching from the one mentioned above, which, if it existed and was prior to the intrusion of the Whin would account for this incongruity, but no trace of this fault could anywhere be detected and its absence was proved in several places.

This section can therefore only be explained by supposing the Whin to cut through the sandstone and its accompanying shale and limestone in the manner indicated in No. 1 Section. The dip of the beds and the presence of folding confirm this, but visible proof is at present wanting.

The fault undoubtedly throws the Whin, but the folding was probably prior to the actual intrusion of the Whin and caused by its approach. This folding would naturally cause weakness and fracture at the shoulders of the folds, which would provide opportunities for the Whin to alter its horizon, and would account for the splitting of the Whin into two or more divisions as is seen in this case. The two sheets are separated by so thin a layer of rock, that it may be inferred that the junction is close at hand.

Proceeding down stream, the Upper Whin (B) is seen dipping some 6° to the east as well as following the general dip of 3°-4° to the S.S.E., and this easterly dip indicates a fold necessary to bring the covering limestone (6) up over Copping Crag. (See No. 2 Section.)

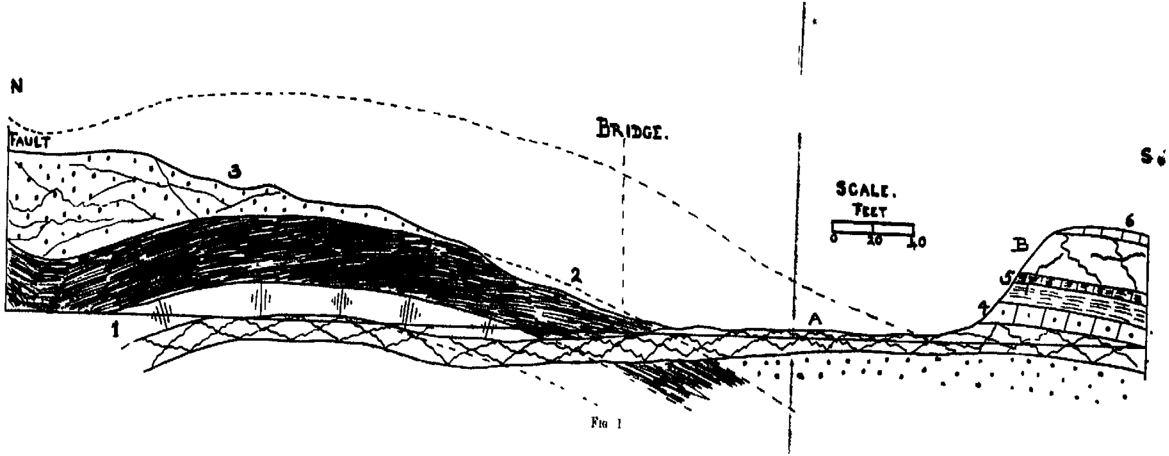
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Fig. 1

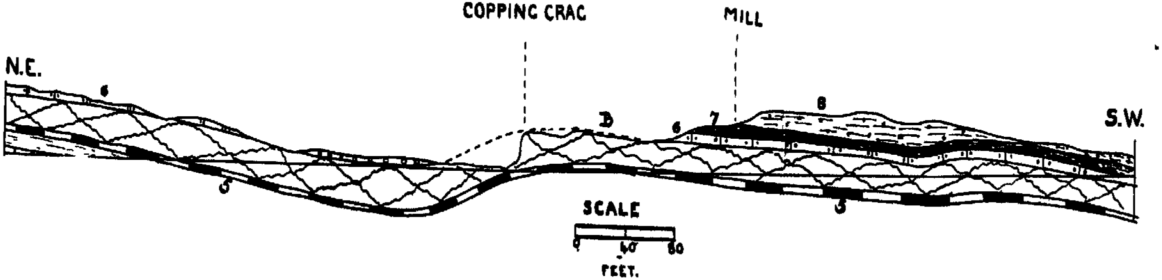
COPPING CRAG
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Fig. 2.

At Copping Crag and at several other smaller folds it is noticeable that the Whin has thickened considerably, as one might expect if the folds were prior to the intrusion

The sedimentary beds follow the same folding. Behind the Mill, a smaller but clearer fold is seen of the Whin and limestone with a certain amount of thickening of the Whin.

Just below the Mill the Whin dips below the covering limestone and is no more to be seen.

Continuing down stream a series of gentle folds bring the limestone (6) down to the stream bed, and here Whin occurs again, but this is a vertical dyke-like off-shoot (B.B B) from the main mass of Whin, which pierces the limestone like a dyke and forms a well defined breccia which marks the north face.

The limestone is succeeded by a black shale (7), a thick sandy shale (8) and a massive limestone probably the "8 yard" (9). The latter is well exposed in a small quarry, where it is pierced by the dyke off-shoot. A beautiful breccia marks the north cheek of the dyke offshoot. (See Photo 2.)

At the western end of the quarry and standing just south of the dyke offshoot, a sharp fold in the limestone is seen, accompanied by a small thrust-plane. The axis of the fold is at right angles to the direction of the dyke, and coincides with the axis of folds mentioned above which are in the direction of the dip, *i.e.*, S.S.E. The fold then was probably prior to the intrusion of the dyke, since if caused by the intrusion one would have expected the axis to coincide with the direction of the dyke

Again at the other end of the quarry, the junction of the dyke breccia and limestone is clear and sharp, and there is no sign of folding.

The dyke-offshoot is some 3 feet broad running east and west (270°) and composed of rock very similar to the Whin, though it is more weathered and there is a complete replacement of the augite and partial replacement of the other minerals by calcite.

The direction practically coincides with the fault mentioned above

The Wansbeck here leaving the escarpment makes a termination of the section

Summary and Conclusions The beds exposed in this area are enumerated below and numbered to correspond with the map and sections Their approximate thicknesses are given —

(9) Massive Limestone	(+14)
(8) Sandy Shale	(8′)
(7) Black Shale	(3)
(6) Limestone	(6′)
(B) Whinsill	(15)
(5) Thin Limestone and Shale	(10)
(4) Limestone	(5)
(A) Whinsill	(+ 3)
(3) Sandstone	(+10)
(2) Shale	(6)
(1) Limestone	(+ 4)

The series includes two sheets of Whin separated by some 15 feet of sedimentary beds and the lower sheet is found changing its horizon through several sedimentary beds

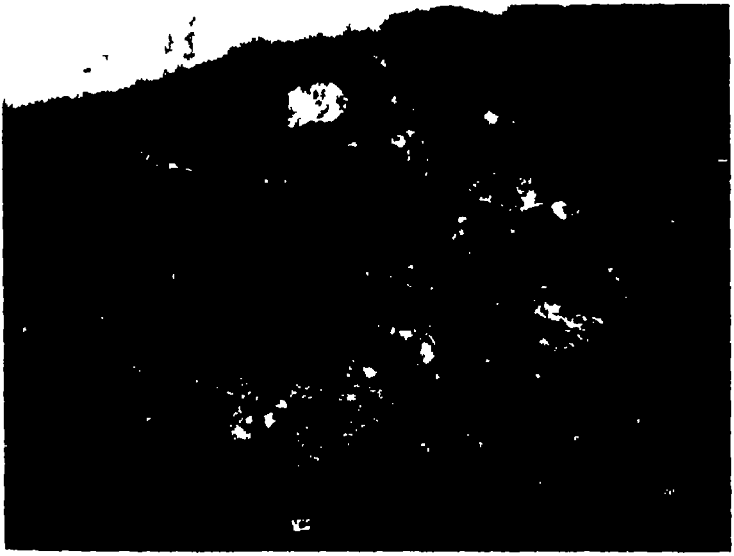
Both the Whin and sedimentary beds are gently folded the axis of the folding being in the same direction as the general dip *i e* S S E

The folding was probably prior to the intrusion and the Whin is found to thicken to dome shaped masses in the folds The shoulders of the folds where steep enough provide opportunities for the Whin to change its horizon

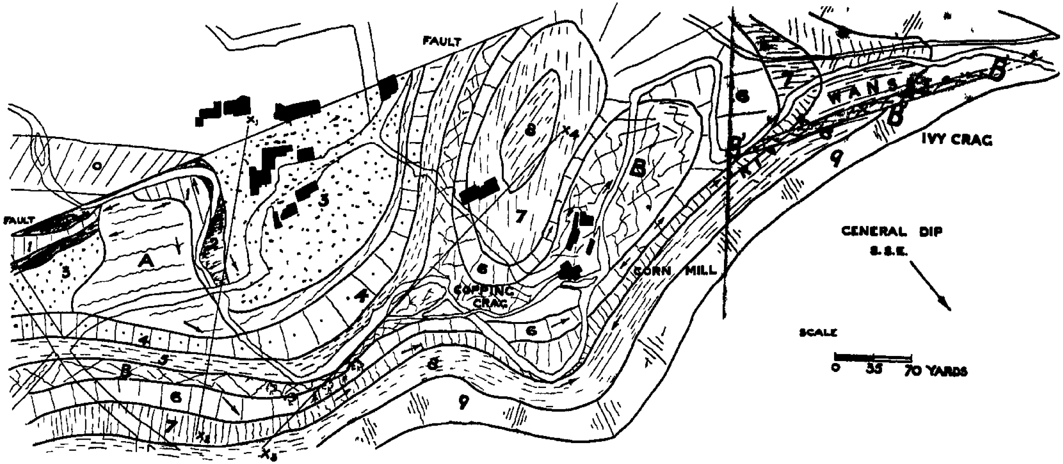
This would seem to be a rational explanation of the continual division of the Whin into separate sheets in this part of its outcrop

The significance of the dome shaped masses is readily understood if we imagine weathering to have taken place dividing the anticlines down to the surface of the Whin and leaving limestone and shale in the troughs This

Showing Fold in Limestone by Thrust x x. Dyke Offshoot is Exposed just on the Right Edge of Photo



Showing Breccia on Southern Cheek of Dyke Offshoot. The White Scratched Surfaces are Saccharoidal Limestone, the rest Whin.


FAULT
FAULT
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would give the irregular line of dome shaped masses with occasional surface breaks which is so characteristic of the Whinsill in Northumberland.

There is also an excellent example of a dyke offshoot from the Whin, which apart from its proximity to the Whinsill might be taken for a true dyke.

Little has been done as regards the weathering and metamorphism of the rocks. The more impure limestones and calcareous shales are in general altered to a very hard porcellanite, breaking with conchoidal fracture described by Teall,[3] and except for the very beautiful minute bedding planes, hardly to be distinguished from fine grained Whin. The Whin found at junctions is invariably very finely grained, and has the augite replaced by calcite, thus causing an effervescent effect with acids.

The purer limestones are usually turned into white saccharoidal, but soft, marbles.

I hope in the near future to be able to apply these observations of the relation of the Whin to sedimentary beds over the country referred to in the introduction between the Tyne and Wansbeck.

In conclusion I desire to express my very hearty thanks to Professor Lebour for his kind criticism, to Dr. Smythe especially for his help in the field, and to Mr. F. H. Walker for his aid in working out the detail of the Section.

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[3] Teall, *Q.J G. Soc.*, 1884.

NOTE ON A NEW ELECTRICAL METHOD OF PREPARING AQUEOUS COLLOIDAL SOLUTIONS OF METALS.

By H. MORRIS-AIREY, M.Sc., and S. H. LONG, B.Sc.

[Read March 13th, 1913.]

The electrical methods hitherto employed in the preparation of colloidal solutions of metals are those of Bredig[1] and Svedberg.[2]

Bredig's method consists in forming a direct current arc between electrodes of the metal to be dispersed, in pure water (conductivity about 3×10^{-6}. If the water is not allowed to become too hot and the current strength is suitably adjusted (5-10 amperes) it is possible in this manner to prepare the hydrosols of many metals. The preparation requires some skill and patience as the arc burns irregularly, and it is difficult to maintain constant electrical conditions in the discharge. This leads to varying sizes of the particles produced, and therefore, to uncertainty as to the colour of the resulting solution.

In the method of Svedberg the oscillatory discharge of a Leyden jar is used to disperse the metal. The discharge can be better controlled, and the conditions made more definite than in the arc method provided care is taken to use very pure water.

It occurred to us that a high frequency alternating arc would provide a method having the advantages of both of the above methods. The large currents which could be used would enable the operation to be carried out quickly, and the oscillatory nature of the arc would enable the conditions to be easily controlled and varied over a very wide range. A suitable generator of the necessary high frequency currents was obtained in the Poulsen arc as used in wireless telegraphy. Leads were taken from two points on the inductance of the oscillatory circuit of the arc, and were led to the terminals of a discharger circuit consisting of an arc lamp, which was so designed as to allow of the arc being struck under water. The inductance and capacity of the oscillatory circuit could be varied, and in the discharger circuit various capacities and inductances could be introduced. These enabled the electrical conditions of discharge to be altered through a wide range The capacity in the Poulsen circuit was varied from 000156 mfd. to 002808 mfd., and the inductance from 90 microhenries to 180 microhenries. This gave a range of frequency from 223,700 per second to 865,200 per second.

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[1] Bredig G., Zeitschr. f. angew. Chem. 1898, p. 951-954.

[2] Svedberg T., Die Methoden zur Herstellung kolloider Lösungen Dresden, 1909

By varying the conditions in the Poulsen circuit as above, and the coupling of the discharger circuit, we were able to obtain currents in the discharger circuit covering a range of from 14 amperes to 15 amperes, and a range of voltage from 480 volts to 4,080 volts. In addition to this, the length of the arc in the discharger could be altered, this being possible owing to the wide range of electrical conditions at our disposal In general, only a few seconds were required to produce colloidal solutions of the following metals: Gold, Silver, Platinum, Palladium, Copper, Lead, Iron, Zinc, Tin, Nickel, Aluminium, Magnesium, Bismuth, Antimony, Cadmium.

Solutions of Carbon were also prepared.

Certain colours had been regarded as charactertistic of the colloidal solutions of many of the metals, and while these could all be produced the flexibility of our method enabled us to show that the colour was a result of the special conditions of the discharge. Where only one or two colours had been previously obtained for a given metal, we found that by suitably adjusting the conditions it was possible to obtain a wide range of colours from several metals.

A remarkable variety was obtained in the case of gold, silver and copper. Three gold colloids were previously known: red, blue and purple, but no single method appears to have been capable of giving all three. The high frequency arc method gives all these and a number of intermediate colours.

An examination of these colloids with a Hardy tube led to the conclusion that the red and blue hydrosols carry charges of opposite signs, while the purple appears to be a mixture of the red and blue colloids Similar results were obtained in the case of other metals, and the electrical and optical properties of some of these are being further examined.

THE STRUCTURE OF METALS.

Abstract of Lecture delivered by Sir J ALFRED EWING, K.C.B , F R.S , at Armstrong College, May 2nd, 1913.

The lecturer began by giving a short account of how the microscope is applied to investigate the structure of metals and what characteristics are revealed, in pure metals, by its assistance. In general the surface had to be prepared for microscopic examination by polishing to remove accidental roughnesses, followed by etching. Etching was necessary because the surface, after polishing, was no longer in the same physical condition as the material below the surface. The surface layer had to be removed before the true nature of the metal could be perceived. To illuminate the exposed face of metal under the lens two methods were followed. The light might be sent down through the microscope tube itself and be reflected up again to the eye: this was called vertical illumination. Or it might come obliquely from the side, grazing the surface of the metal.

When a metal which could be obtained in a pure or nearly pure state was polished and lightly etched, the first thing seen in the microscope was that the exposed surface was divided into irregular areas called grains, as various in form as the counties on a map of England. The boundaries between the grains appeared under vertical illumination as dark lines, with no pretence of geometrical regularity: the grains seemed quite casual both in shape and in size. Under stronger etching a difference in texture could be discerned between one grain and another, which became very obvious when the illumination was oblique. Some of the grains then appeared very bright in the field of view, others very dark, and others of intermediate shades. On turning the specimen so that the light fell on its surface from different directions the brightness of any one grain underwent striking variations These features were illustrated by lantern slides showing microscopic photographs, and by a specimen of cast lead in which the grains were so large as to be readily seen without magnification The brightness, though varying widely from grain to grain, was constant over the exposed surface of any one grain Examination under high power showed that this was due to the fact that the surface of each grain was made up of a multitude of little parallel facets which acted like mirrors in reflecting the light These faced one way all over the grain, but faced different ways in different grains

The explanation was that each grain was in reality a crystal, notwithstanding the complete irregularity of its external form The essential characteristic of crystalline structure was not geometrical regularity of the external form but of the internal arrangement of the units out of which the crystal was built up Its crystalline quality made it appear as if composed of brickbats, like a block of brickwork, and the effect of etching was to remove certain brickbats from the surface, leaving the faces of others visible The irregular boundaries of the grains were due to the interference between each grain and its neighbour during the process of growth

To make the process intelligible, they might think of what would happen if a number of fairy children were set down in a nursery and provided with an unlimited supply of brickbats all exactly alike Each child began building, with complete freedom as to where it put down its first brickbat, and in what direction, and the rate of building might vary But the ultimate result was that the whole space became filled, for each child had to stop building only when its pile came into contact with those of the others Throughout each pile the brickbats lay parallel to one another but the directions were as a rule quite different in different piles If a section through the whole space were taken, and the surface brickbats picked out, the appearance would be exactly the same as that of the grains of a metal when cut and etched. In some cases the child might take possession of a comparatively large territory by building out branching arms forming a skeleton to be filled in at leisure. This was analogous to what in crystallography was called dendritic growth, examples of which were often found in metals.

Other evidence of the crystal nature of the grains was given by means of photographs showing "air-pits" in the surface of a specimen of cadmium cast against a smooth glass plate. Microscopic cavities formed by air or by gas given out during the solidification of the metal took a geometrical form, and when a group of such cavities occurred on the surface of one grain their forms were similar and parallel They resulted, as it were, from brickbats being omitted in the building

The lecturer next discussed the behaviour of the crystal grains when the metal was strained beyond its elastic limit. Plastic straining was possible without destruction of the crystalline character of the grains A specimen of metal stretched until it broke in the testing machine, or a bar rolled in the cold state from a large to a small section, showed grains that were much distorted, but the brickbats revealed by etching were still parallel to one another over the face of each grain. This was because the alterations of form took place by means of slips occurring on parallel planes within each grain, like the slipping of cards over one another in a pack. When the exposed surface of a grain after straining was viewed in the microscope, the traces of these slips were seen as parallel lines or rather very narrow bands to which the name of slip-bands had been given. By using oblique light it was shown that these markings on the surface were really steps, each of which was caused by one portion of the crystal grain slipping over the adjacent portion through a short distance. In general there were at least three such systems of planes of slipping, corresponding to certain natural planes within the crystal in which slipping took place most easily. 'The combined effects of slip occurring on three or more planes was to allow the grain to assume an entirely different shape, but without losing its crystalline character Examples of slip bands were shown in iron, lead and other metals, and attention was called to the herring-bone pattern assumed by slip bands in cases where the grains exhibited the peculiarity known to crystallographers as twinning It was explained that when a strained metal was annealed a recrystallization occurred though the metal remained throughout the process at a temperature much below its melting point

The lecturer then discussed the possible configurations of the elementary particles or structural units of the grains, which would be consistent with the observed crystalline quality The units must be grouped in what Kelvin had called a homogeneous assemblage They were equally spaced in rows, and the rows in layers In a model this characteristic could be secured by representing the units as spherical balls of the same size There were three possible ways in which such balls could be piled so as to satisfy the condition of cubic symmetry which was met with in iron, lead copper, gold, silver and many other metals In the most open mode of piling each sphere was in contact with six others In another mode each sphere was in contact with eight others In a third mode which was the closest possible kind of piling, each sphere was in contact with twelve others It was shown by means of models how these modes of piling gave cubic symmetry, and how in a modification of the third mode twin groupings could be formed Probably the structural unit was itself a very complex group of atoms or electrons, exerting mutual forces between each unit and its neighbours which brought the units into place and held them there, and accounted for the dissipation of energy that occurred when there was slip Except for these forces one might conceive the unit as free to turn, and under strain it probably did turn a little way before the elastic limit was passed when the original bonds between it and its neighbours were broken The effects of elastic and non-elastic straining could be indicated by means of models in which each unit had poles exerting forces on corresponding poles in its neighbours. Various models of this kind were exhibited and their action briefly discussed

Particulars of observations referred to in the lecture will be found in the following papers:

Experiments in Micrometallurgy Effects of Strain, by J A Ewing and W Rosenhain. *Proc Roy Soc.* Vol 65, 1899.

The Crystalline Structure of Metals Bakerian Lecture, 1899, by J A. Ewing and W Rosenhain. *Phil. Trans Roy Soc* Vol 193, 1899.

The Crystalline Structure of Metals (Second Paper), by J A Ewing and W Rosenhain *Phil. Trans. Roy. Soc* Vol. 195, 1900

The Fracture of Metals under Repeated Alternations of Stress, by J A Ewing and J W. Humfrey. *Phil. Trans Roy Soc* Vol 200, 1903

Effects of Strain on the Crystalline Structure of Lead, by J W Humfrey. *Phil. Trans Roy. Soc* Vol 200, 1902

The Plastic Yielding of Iron and Steel, by W Rosenhain. *Journ Iron and Steel Inst* 1904.

Deformation and Fracture in Iron and Steel, by W Rosenhain *Journ Iron and Steel Inst* 1906.

Presidential Address to the Engineering Section of the British Association York, 1906, by J A Ewing *Rep Brit Assoc.* 1906.

The Structure of Metals (The Wilde Lecture), by J. A Ewing. *Manchester Memoirs* Vol 51, 1907

The Inner Structure of Simple Metals (The 1912 May Lecture), by J A Ewing *Journ Inst of Metals* Vol 8, 1912

Mathematics and the Inductive Methods of Logic.

By Godfrey H. Thomson, D.Sc., Ph.D.

[Read May 8th, 1913.]

> Ich glaube nicht, dass ich viel eignes Neues lehre,
> Noch durch mein Scherflein Witz den Schatz der Weisheit mehre;
> Doch denk' ich von der Müh mir zweierlei Gewinn:
> Einmal, dass ich nun selbst an Einsicht weiter bin;
> Sodann, dass doch dadurch an manchen Mann wird kommen
> Manches, wovon er sonst gar hätte nichts vernommen.
> Und auch der dritte Grund scheint weit nicht des Gelächters:
> Dass wer dies Büchlein liest, derweil doch liest kein schlechters.
>
> (Rückert, Weisheit des Brahmanen.)

Introduction

I propose to begin this paper by defining more accurately than can be done in a title the ground which I intend to cover, and by mapping out in advance the main lines along which I intend to proceed. In the first place, then, I use the word Logic in the ordinary way in which it will be used, I think, by anyone who approaches the subject from the scientific side. That is to say, while I recognise that Logic is the science of reasoning, I do not find it easy to join those who, to quote Mr. Sidgwick[1], make a deliberate attempt to keep Logic purely formal, and I shall not try to regard the process of reasoning as something distinct from the subject matter about which it is employed. It would indeed be almost impossible for me to do so, for what I propose to do is to show that a certain method of reasoning which has been found necessary in biology, and is coming into use also in some other sciences, and which involves the use of the calculus of probability, is an extension, in practice if not in principle, of the inductive methods of logic, and this new method must be explained in the language of these sciences and by means of illustrations from them.

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[1] A. Sidgwick, *Use of Words in Reasoning*, p. 9.

It is, perhaps, unnecessary to explain that by induction I do not mean what is called "mathematical induction."

In induction the causal connection of two or more occurrences is sought for empirically by observation. One occurrence is focussed, and the occurrences causally connected with it are required. By causal connection I mean merely uniformity of association in the phenomenal world.

The methods of induction are used by everyone in daily life. They were in a sense already formulated by Aristotle; they were tremendously emphasised by Bacon; and by John Stuart Mill they were given the form in which they are to-day commonly quoted. Mill's four experimental methods would need no amplification whatever, were the different events of nature really as easily separable as the Roman letters he uses as symbols for them [2]

QUALITY AND QUANTITY.

In actual fact, however, Mill's methods are not so easily applicable There are two cases to be examined, the second of which is again sub-divided into two.

(1) The occurrence κ, the cause of which is to be investigated, is sharply marked off from not-κ.

(2) More commonly, however, κ is some quantity capable of graduation,[3] there may be much or little of κ. It may, for example, be the temperature of a room or the height of a man.

> (*a*) We may then by the use of a standard[4] convert this case artificially into the former. For example, we may say that when a man is taller than a certain post he is *tall*, otherwise he is *not-tall*. Such a standard can often be used also in dealing with mental

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[2] In truth it may be doubted whether they are separable at all except arbitrarily by using a standard.

[3] It may be doubted indeed whether case (1) ever really exists, or whether we are not always using a standard, unconsciously, to separate a quantity into two so-called qualities See footnote, page 86

[4] See J Venn, *Empirical Logic*, London, 1889, ch 18 and 19

phenomena. Moreover, more than one standard may be chosen, and κ divided into three or any number of classes.

(*b*) If the standard is such that we can mark where it reaches to, and re-apply the same standard, beginning at that point, it becomes a unit, and we can measure κ to that degree of accuracy which our means permit or our necessity demands This we can do with the height of a man. We cannot do so, however, with mental phenomena. For example, we cannot say that one room *feels one and-a-half times as hot as another*, although we can measure the expansion of mercury in the two rooms. Only by making Fechner's assumptions can a kind of measurement be carried out in such cases.

We may call these three cases the cases of

(1) Simple Presence or Absence;
(2) Continuous Variation with (*a*) Classification by standards; (*b*) Measurement by a unit.

Let us suppose now that an event α can be identified among the antecedents, and that somehow or other, either by casual observation, or by analogy, or by a half complete deduction from known laws, we have come upon the notion that α may be, as we say, the cause or part of the cause of κ[1]. This event α may also be brought under one of the above three cases: and in investigating the causal connection of α and κ there may occur, therefore, any of the following combinations:

i Both are simply present or absent
ii Both are continuously variable, and
    (*a*) each is classified by its standard or standards;
    (*b*) each is measured by its unit;
    (*c*) one is classified and the other measured.

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[1] The question of how we come in the first place to have any suspicion that α and κ are connected seems to me to be one rather for psychology than logic In any case this paper is concerned with the scientific making of inductions, not with those first faint glimmerings which lead us to try along certain lines. I shall, however, return to this point later. *Somehow* we suspect α to be causally connected with κ.

iij One is simply present or absent, and the other is variable and
- (*a*) classified by a standard or standards;
- (*b*) measured by a unit.

ANALYSIS OF QUALITATIVE METHODS.

If the pair of phenomena we are investigating come under case i (both simply present or absent), then Mill's Methods of Agreement, Difference, and the Joint Method of Agreement and Difference, are frequently applicable. I wish to introduce a manner of tabulating these methods which will make what follows clearer, and would, I think, be useful in teaching the subject We examine sets of antecedents and consequents, and record each in the proper compartment of what we shall call a Fourfold Table. For example, consider the case—

Antecedents.	Consequents
ABC	*abc*
ADE	*ade*
AFG	*afg*
AHK	*ahk*

to which the Method of Agreement is applicable, and examine the connection of a=A, and κ=a. All four cases will be recorded in the first compartment of this table (Fig. 1).

FIG. 1.

	$\kappa = a$	not κ
a = A	4	0
not A	0	0

Method of Agreement. Everything changed except a.

If the following sets of antecedents and consequents be similarly tabulated:

Antecedents	Consequents
ABC	*abc*
BC	*bc*

one case comes into the first and one into the last compartment (Fig. 2). This is the Method of Difference where it is

Fig. 2

	$\kappa = a$	not κ
$a = A$	1	0
not a	0	1

Method of Difference. Nothing changed except a.

possible to remove *a* leaving everything else unchanged. If it is impossible to remove *a* without changing several other antecedents then more cases must be tried. All of them will come into the same two compartments (Fig. 3).

Fig. 3

	κ	not κ
a	50	0
not a	0	50

Joint Method of Agreement and Difference. a cannot be changed without several things being changed, but the only invariable difference is a.

This is the Joint Method of Agreement and Difference. If we take the cases in pairs one from each compartment, the members of any one pair differ from one another in several ways, but the only invariable difference is α.

As an example upon which to use such a Fourfold Table let us take the following from an elementary text book of logic[6]:—

Antecedents	Consequents
ABDE	*stpq*
BCD	*qsr*
BFG	*vqu*
ADE	*tsp*
BHK	*zqw*
ABFG	*pquv*
ABE	*pqt*

and let us suppose that we are behind the scenes and know that *A* is the cause of *p*, *B* of *q*, *C* of *r*, and so on. An observer who does not know this[7] is led to suspect a causal connection between *A* and *p* and enters the cases into a Fourfold Table thus (Fig. 4).

FIG. 4.

	$\kappa = p$	not κ
$\alpha = A$	4	0
not α	0	3

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[6] Jevons, *Elem. Lessons*, p. 328.

[7] An observer who had the events as separate as ABC would already have accomplished most of his task.

If he examines not only these seven but many cases and each one as it comes up fits into one of these compartments, then the probability becomes very great that *a* is both necessary and sufficient to cause *κ*, and by examining a sufficient number of cases he can attain to any degree of probability which he may decide upon as being for practical purposes certainty. As soon however as a case crops up which must be put into the third compartment he knows that *a* is not necessary though it may be sufficient: as soon as a case crops up in the second compartment he knows that *a* is not sufficient though it may be necessary. The first of these is Mill's case of Plurality of Causes. The second Mill does not state: I shall call it the case of Plurality of Effects[8]. If cases occur in all the compartments *a* is neither necessary nor sufficient, there is either no connection at all or there is Plurality both of Causes and Effects.

Such Plurality is in many cases certainly not real but apparent. If several causes appear each to result in the same effect, if (to use Mill's example) combustion, friction, electricity, can each cause the feeling of warmth, then these apparently different things may have something in common which causes that effect: and similarly if on different occasions the same cause gives rise to different effects, then these effects may have something in common which is the real effect of that cause, while the differences are to be otherwise accounted for. In the first case the apparently different causes contain something in common which is the real cause: in the second case the apparently identical causes are really different. It does not do to press this explanation of Plurality too far however, or we find ourselves merely restating the problem in other words, saying in fact that "what these various causes have in common is the power of causing this common effect." If a bell in the kitchen can be rung independently from two rooms by separate wires there are two causes which, at any rate for practical purposes, have nothing in common. Yet the scientist must always be on the lookout to discover something in common between causes which have the same effect or effects which have the same cause.

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[8] It is, of course, quite different from Mill's Intermixture of Effects.

Our example will serve to illustrate all the case of apparent Plurality. Let us suppose that among the antecedents the combination (*AE*) is peculiarly striking and arrests the attention of the observer and that he suspects it to be the cause of *p*. He does not recognise that *A* is part of it. He then gets the following Fourfold Table (Fig. 5). (*AE*) whenever it occurs is sufficient to cause *p*: but several other causes, as (*AF*) or (*AD*) are equally sufficient.

FIG. 5.

	$\kappa = p$	not κ
α = (AE)	3	0
not α	1	3

Plurality of Causes. α sufficient but not necessary.

Let us on the other hand suppose that although the observer can recognise *A* he has been led to suspect that it is causally connected with (*pq*) which he does not recognise to be complex *p* and *q* might be gases which he does not recognise but which cause an explosion when both are present. He then gets the following Table (Fig. 6).

A is necessary to produce (*pq*): yet sometimes *A* is followed by other effects and (*pq*) is absent, and he does not recognise that these all have *p* in common.

Fig 6

	$\kappa = (pq)$	not κ
$\alpha = A$	3	1
not α	0	3

Plurality of Effects α necessary but not sufficient

Finally, combining these cases suppose that the suspected antecedent is (AE), the suspected consequent (pq) We then get Fig 7

Fig 7

	$\kappa = (pq)$	not κ
$\alpha = (AE)$	2	1
not α	1	3

Plurality both of Causes and Effects α neither necessary nor sufficient.

In this last case we should of course conclude that α was neither necessary nor sufficient to cause κ. But if as we examined more and more cases the preponderance of them continued to fall into the first and last compartments we should suspect that nevertheless α and κ were somehow causally connected, that is to say that some part of α (here the part A), was necessary and sufficient to ensure the presence of some part of κ (here the part p)

The Method of Agreement and Disagreement

These last three cases may conveniently be classed together under one method which as it has not yet so far as I know received a name I shall call the Method of Agreement and Disagreement and I may in imitation of Mill, define it in the following Canon

If two groups of events are such that the only invariable difference is the presence or absence of a certain circumstance and if each of these groups be divided so that the only invariable difference between the parts is the occurrence or non occurrence of a certain phenomenon and if as the number of events is increased the ratio of the parts of the one group does not tend to become equal to the ratio of the parts of the other group then this phenomenon is connected with that circumstance by some fact of causation

For if they be not connected there is no reason why the group containing the circumstance in question should contain a proportion of occurrences of the phenomenon different from that which is found in the other group, and we should have (Fig 8)

$$\frac{x_1}{x_2} = \frac{x_3}{x_4}$$

or

$$x_1 x_4 - x_2 x_3 = 0 \qquad (1)$$

FIG 8

	κ	not κ
a	x_1	x_2
not a	x_3	x_4

The Method of Difference, and the Joint Method, are special cases of this Method of Agreement and Disagreement, and the Method of Agreement is a portion of it. The essence of all these methods is not that cases can be found which go into x_1 or x_4, *but that no cases can be found which go into x_2 or x_3*. In that case a high degree of probability is soon attained. In cases where some of the events fall into x_2 or x_3 *we can attain just as great probability* provided we can collect a sufficient number of instances, for here more are required. The number required depends on the proportion of instances found in x_2 and x_3: for if few are found there, the ratios x_1/x_2 and x_3/x_4 are very different from one another and it is soon clear that they are not tending to become equal. In this case the causal connection is close

The problem of measuring how close it is, is a problem in contingency.[9] We shall see later how to deal with it in case ii(*a*), where it is assumed that below the arbitrary division into classes lies a continuous variation.

QUANTITATIVE METHODS.

So far we have been considering case i where both a and κ are simply present or absent. In all the other cases one or both of these is continuously variable. All these cases can be reduced to case i by means of a standard or standards. The distribution of numbers in a Fourfold Table will now, however, depend on the choice of these standards.

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[9] See Karl Pearson, "On the Correlation of Characters not Quantitatively Measurable," *Phil Trans Roy Soc.*, 1900, Series A, cxcv., 1. Udny Yule has suggested a certain co-efficient of association which is easily calculated and which he claims to be a suitable measure This assumes discrete "presence and absence" See footnote, page 77 of this article. A controversy has raged between Pearson and Yule on this point. Yule's views are indicated in his *Introduction to the Theory of Statistics*, Griffin, 1912, where references to papers by himself and others are given In the writer's opinion, Pearson's views are more correct See also Karl Pearson, "On the Theory of Contingency and its Relation to Association and Normal Correlation" *Drapers' Company Research Memoirs*, Biometric Series, I, 1904; D. Heron, "The Danger of Certain Formulæ suggested as Substitutes for the Correlation Co-efficient," *Biometrika*, 1911, VIII, 109; G. U. Yule, "On the Methods of Measuring the Association between two Attributes," *Journ. Roy. Stat. Soc.*, 1912, LXXV; Karl Pearson and D. Heron, "On Theories of Association," *Biometrika*, 1913, IX, 159.

As an example let us take the question of the inheritance of stature from father to son. Stature of course admits not only of standardised classification but also of real measurement, but it is easy to imagine cases where it might be impracticable to do more than pass a judgment tall or not tall - for example an explorer among savages might thus collect some evidence by inducing natives to compare themselves with him, though they might be unwilling to submit to measurement. Of 1,078 pairs of English fathers and sons who were measured in an investigation by Karl Pearson, there were, if we take 5′ 9½″ as standard the following numbers of tall and not tall sons and fathers (Fig. 9).

Fig. 9.

	Not tall	Tall
Not tall	587·25	100·75
Tall	209·75	180·25

Sons

Fathers

(The fractions arise through certain individuals being equal to the standard.)

Here x_1/x_2 is equal to 5·8 while x_3/x_4 is equal to about 1·2, that is they are still very different although a large number of cases have been examined, so that we suspect a causal connection, though we are not yet able to give any measure of the closeness of this connection, which shall be reasonably independent of the arbitrary choice of standards. Such a measure will be explained later.

The method given by Mill for studying variable phenomena is the Method of Concomitant Variation which is the quantitative form of the Method of Agreement. In this we are instructed to vary the amount of a without changing any of the other antecedents, and to observe whether κ varies in any way in sympathy, either directly, inversely, periodically, or in any more complex fashion.

The difficulty here as before is, of course, that we cannot as a rule be sure that the cases observed differ only in the amount of a present and in nothing else. In fact we are usually sure that this is not so In certain sciences however a very close approach to this ideal can be made, namely in the experimental sciences of physics and chemistry. There we can as a rule so manipulate the antecedents of an occurrence that certain of them remain almost constant, while those to be studied undergo large changes. In physics therefore we attain a mathematical connection[10] between a and κ, for example we may find, say,

$$a = m\kappa.$$

In the biological sciences, on the other hand, the case is very different. In these and in other sciences in which experiment is difficult and observation the usual method, it is almost impossible to change one antecedent without changing many other antecedents at the same time, often to an unknown extent. This may be, as for example in experimental psychology, because the antecedents themselves are bound together by as yet unknown laws; or it may be, as for example in agriculture, because some of the antecedents are completely beyond our control. In such cases appeal has to be made to a very large number of observations indeed, and the results have to be considered by a special method the study of which is a branch of the calculus of probability.

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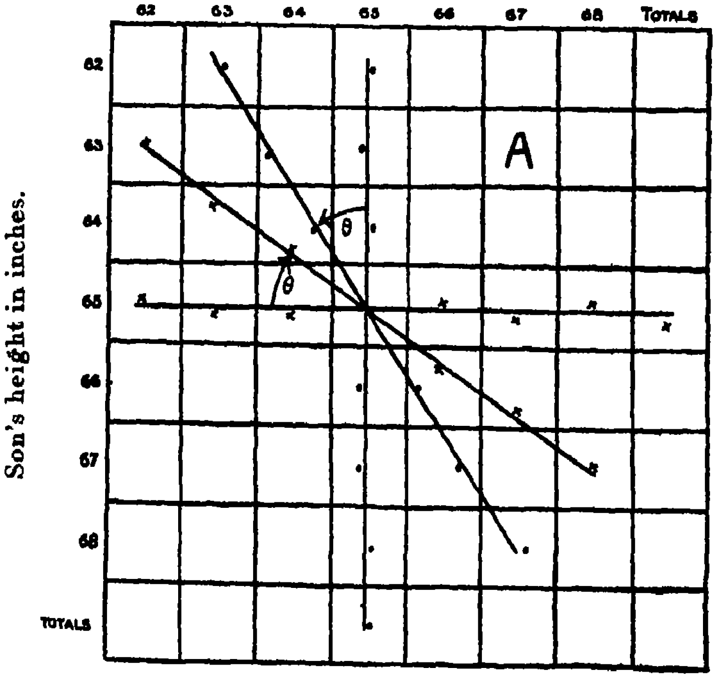
[10] There will be, it is true, always some slight deviation from the result in an actual measurement, and appeal is again made to the calculus of probability in ascertaining whether the observed error is explicable by the remaining small unrecorded "chance" changes in the other antecedents In careful experimenting, however, the deviations are comparatively small.

The Method of Correlation.

This Method is the quantitative form of the Method of Agreement and Disagreement, just as the Method of Concomitant Variation is the quantitative form of the simple Method of Agreement. I take again in illustration the question of the inheritance of stature in man. Stature

FIG. 10.

Father's height in inches.



admits of measurement with a considerable degree of accuracy. Let us suppose that a large number of fathers and adult sons have been measured and the results put into a table ruled in this way (Fig. 10). For example if a father's height is 67 inches and his son's height 63 inches this case will go into the pigeonhole marked *A*. Suppose now that the average height of all the sons is 65 inches, represented by the cross in the last column. Next fix the attention upon the sons of 64 inch fathers, represented by the third column. If there is no connection whatever between height of father and son this column will be an ordinary sample of sons and the average will also be about 65 inches. Similarly with every other column. Thus a line will go horizontally across the figure In the same way by taking the fathers in rows belonging to different heights of son, the vertical line of dots will be got if there is, as we are for the moment assuming, no connection between father's height and son's height.

In the actual case, however, there is a connection; for tall fathers often have tall sons and short fathers short sons. The average of the column of sons belonging to 62 inch fathers would as a matter of fact be less than 65 inches, and the line of crosses, instead of being horizontal, would lie along the sloping line shown (Fig. 10), inclined to the horizontal at an angle θ. In the same way the vertical line of dots will in the actual case lean over from the vertical at an angle. I shall show later that these angles are not as a rule the same, but for the present let us suppose this angle also is θ. Then the correlation factor which measures the degree of connection between father's and son's heights is $r = \tan\theta$.[11]

If there is no connection, θ and therefore r will be zero. As the degree of connection is increased θ increases and the sloping lines gradually approach each other. When they meet $\theta = 45^\circ$ and the correlation factor r equals unity. This is the usual linear physics graph in which as a rule two lines are not seen. But in work in which large extraneous influences cannot be prevented the two lines are evident. They are known in biometry as regression lines. Fig. 11 shows an actual correlation table connecting father's and son's stature. It is taken from an article by Karl Pearson and Miss Alice Lee[12] in Biometrika II. 1902-3 p. 415.

[11] Francis Galton, *Journ Anthrop Inst*, 1886, xv, 246; *Proc. Roy. Soc.*, 1886, xl, 42, and 1888, xlv, 135.

[12] Since I wrote this article I have discovered that Mr. Udny Yule has used the same table as an illustration in his book on the Theory of Statistics.

Fig 11

Father's Stature

Son's Stature

Inches.	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75
60					2	2	4										
61					2				4								
62		1	1		2	4	1	1	2	2							
63		1	1	9	9	8	16	20	11	5		1	1				
64	4		6	15	12	17	32	37	12	5	6	3	5				
65	8	4	2	8	13	38	4	43	30	22	14	10					
66		2	4	9	21	38	40	67	7		21	8	10	4			
67		6	8	19	14	5	7	16	15	78		5	13	2	4		
68			6	8	30	4	41	7	1	4	118	9	34	38	9		
69			4		21	20	1	73	4		1	9	4	14	9		4
70					4	10	23		7	74	0	78	6	25	14	6	4
71						13	20	35		7	9	99	53	32	20	4	4
72						1	12	5	28	31	3	4	4	34	11	2	
73							3	3	10	30	6	24	30	25	13	2	2
74					4		6	6		21	9	10	26	13	13		8
75										4	8		10	3	7	2	
76										5	1		2	4	4		
77										5	1	4			6		
78											4	4		1	3		
79														1	1		

For simplicity in printing the numbers in the original table in *Biometrika* 1902 3 ii 415 have been multiplied by four this eliminates quarters and halves which occur through some heights being half way between whole inches.

It is interesting to look at a correlation table in greater detail If we think of it as a plane horizontal surface and erect over the centre of each compartment a vertical line proportional to the number written in that compartment then the tops of those lines touch a surface, the "correlation surface," in shape something like a bell with an oval mouth.[13] Its contour lines are ellipses and if there is no correlation these are arranged with their axes vertical and horizontal, whereas if there is correlation the ellipses lie obliquely

In Figure 11 the numbers over 40 have been printed in italics so that this contour line can be approximately traced. It is seen to be almost an ellipse, and it lies obliquely. Any vertical section of the surface gives a bell shaped curve known as the probability curve. The totals of the rows or of the columns are also arranged in such bell shaped curves[14] which are not usually identical with each other. It is for this reason that the regression angles (which we put each equal to θ in our elementary discussion) are not really equal and

$$r = \sqrt{\tan\theta_1 \tan\theta_2}\,.$$

When correlation is complete the lines close up on each other, though not necessarily at 45°, especially where the qualities correlated are of different natures

It will be noticed that I have assumed that the regression lines are linear and not curved. This is in fact the usual case but where it is not so, other mathematical devices have to be used.[15] The actual determination of correlation factors is no longer made by the geometrical method which I have indicated, but by various formulae (of which the Bravais-Pearson formula[16] is the chief) and with the help of tables published chiefly in Biometrika We are now in a position to return to the case dealt with on page 78 viz case ii(b) where the two phenomena are continuously variable but are only classified not measured The numbers of cases which will appear in the four compartments of a Fourfold Table will then depend upon the choice of standards and a method of calculating the correlation factor is required which will give the same result (though not the same accuracy) wherever the standards are chosen

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[13] A model was shown at the meeting. Mr. Udny Yule, in his *Introduction to the Theory of Statistics* gives a diagram of the ideal normal frequency surface in fig 20 and in fig 30 he gives a diagram of the actual surface for the above correlation table

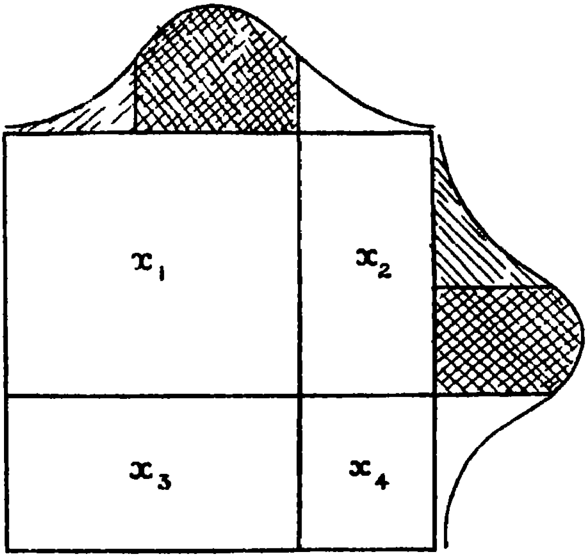
[14] See Figure 13.

[15] A correlation *ratio* η is then calculated See Karl Pearson, "On the Theory of Skew Correlation and Non-linear Regression," *Drapers' Company Research Memoirs*, Biometric Series, II

[16] A Bravais, "Analyse mathématique sur les probabilités des erreurs de situation d'un point." *Acad des Sciences, Mémoires presentés par divers savants*, Series II, 1846, IX, 255; Karl Pearson, "Regression, Heredity, and Panmixia," *Phil Trans.*, A. 1896, CLXXXVII, 253.

This diagram represents such a table

FIG 13



The two bell curves represent the total distributions of a and κ respectively We then have

$$\frac{(x_1 + x_3) - (x_2 + x_4)}{\text{N (the total no)}} = \sqrt{\frac{2}{\pi}} \int_0^h e^{-\frac{1}{2}x^2} dx$$

(the *cross* hatched area on the upper curve)

and

$$\frac{(x_1 + x_2) - (x \quad + x_4)}{\text{N}} = \sqrt{\frac{2}{\pi}} \int_0^k e^{-\frac{1}{2}y^2} dy$$

(the *cross* hatched area on the side curve)

h and k can then be found from tables, and then[17]

$$2\pi \frac{x_1x_4 - x_2x_3}{N^2 e^{-\frac{1}{2}(h^2+k^2)}} = r + \frac{r^2}{2}hk + \frac{r^3}{6}(h^2-1)(k^2-1)$$
$$+$$

Tables have been published which make the whole operation fairly easy. For the case of fathers and son's stature already given in Fig. 9, for which the complete correlation method gives $r = 0.514 \pm 0.015$ this method gives $r = .509$ to a first approximation. The probable error is *much larger* than 0.015.

Methods similar to this have also been worked out by Pearson and his students for many of the other cases given on page 78.[18]

THE MECHANICAL NATURE OF INDUCTION AND THE INDUCTIVE MACHINE

It is important to notice that very often, even after the cases have been collected and arranged in a table, the worker is yet quite uncertain whether any causal connection is present. But a mechanical application of the formulae of correlation will tell him whether the cases indicate any such connection, how great the connection is, and what probability is to be attached to its correctness. In fact, we have here a machinery for induction just as Jevons made a machine for deduction,[19] and it is in this sense that the correlation method seems to be new. It is almost Bacon's hope realised, of being able to make inductions mechanically.

It is easy to see how such a machine could be actually constructed for a Fourfold Table to indicate merely whether $x_1/x_2 = x_3/x_4$. If we arrange a kind of "cash register" machine with four compartments, each with a key or handle, it would be possible to arrange on the principle of a calculating machine that the necessary divisions would occur mechanically. We would then examine case after case, and for each one turn the handle in the proper compartment, and as we went on the indicator above would show the values of x_1/x_2 and x_3/x_4. To build such a machine for the complete method so as to show r direct, and for a complex correlation table would be difficult, but is not inconceivable, indeed the series of articles and tables which Pearson has published in Biometrika are in effect such a machine, and can be used mechanically as a recipe for grinding out inductions.

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[17] Note that when $x_1x_4 = x_2x_3$ r will be zero and compare page 85.

[18] For an excellent summary and bibliography, see W. Brown, *Mental Measurement*, Cambridge, 1911.

[19] Raymond Lully, an Italian, invented in the thirteenth century a contrivance by which logical notions could mechanically be brought into combination. Bacon and Descartes both aimed at inventing a general and certain method of research, and Leibniz gave much time and thought to the idea of a "calculus ratiocinator." *V. Leibniz*, by J. T. Merz, Edinburgh, 1884, p. 106.

It is unnecessary to say that even were such a machine perfected there would still be any amount of room for the individual intelligence in deciding upon what material and in what manner it was to be used, and in interpreting its results. Nor must it be supposed for an instant that skill in experiment and care in observation and selection would be any less important: it is, on the contrary, of the greatest importance to do everything possible to bring the disturbing antecedents to as constant a state as possible. I may here return to a point which I mentioned in passing above. It must be remembered that before all this mathematical calculation is commenced the inductive leap has already been taken, that the material has been collected and the calculations carried out in the light of a hypothesis which has already been made, and which is only being tested. This is undoubtedly the usual case, but the objection applies as much to the ordinary experimental methods of Mill. In other cases, however, the inductive act certainly happens as a result of the calculation, though it is, of course, a totally different thing from the calculation. I do not urge the case where the experimenter expects that a will vary directly with x and, on finding a negative correlation, is forced to conclude that it varies inversely, for the directly negative may, I suppose, be included in the positive induction. But in calculating the correlation between a number of inter-connected phenomena it is rare that new connections do not dawn on the mind. This is particularly the case in experimental psychology where the interconnection of the various quantities measured is but vaguely understood. Often, indeed, a number of phenomena are measured, and all the possible correlations and partial correlations between them worked out, in the hope that some definite law will crop up in the process. When a number of correlations have thus been calculated, it is possible to calculate what are called partial correlation coefficients.[20] This device bears the same relation to the Method of Correlation as does Mill's Method of Residues to his ordinary methods. Mill's fourth Canon runs as follows: "Subduct from any phenomenon such part as is known by previous inductions to be the effect of a certain antecedent, and the residue of the phenomenon is the effect of the remaining antecedents."

Consider a case where three phenomena, a κ and ω are suspected to be causally connected. The correlations of a and κ, of κ and ω, and ω and a have been calculated. We wish then to know for example whether a and κ are connected by the same fact of causation as that which joins each of them to ω, or whether they are connected independently by one fact of causation, while other and different bonds unite each of them with ω. To help us in this point we calculate what the correlation between a and κ would be were it possible to keep ω constant.

The formula is

$$r_{a\kappa\cdot\omega} = \frac{r_{a\kappa} - r_{a\omega}\cdot r_{\kappa\omega}}{\sqrt{\left(1 - r^2_{a\omega}\right)\left(1 - r^2_{\kappa\omega}\right)}}.$$

I take an example from some calculations which I made two years ago on the examination results of 26 students of this college in a number of subjects. The correlation factors are given in Fig. 14, (they are subject to large probable errors). If we now calculate what would be the correlation between marks in History and Literature were ability in the Theory of Education to be the same for all, we find 0·21, much less than the "raw" correlation 0·71, showing that there is something in common between all three subjects. To a less extent this is true of all the subjects, probably, and at the time I was curious to know if this common factor in examination success was power of cramming. I gave these students, therefore, a test in rapidity of memorising nonsense syllables, and worked out the necessary correlations and found that this power was *not* the common factor, the examinations were testing something more than this kind of memory.

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[20] G. Udny Yule "On the Theory of Correlation for any Number of Variables, treated by a New System of Notation," *Proc. Roy. Soc.*, Vol 79 A, pp. 182-193, 1907.

FIG. 14.

	Educational Theory	History	English Literature	Geology	Hygiene	Weight per Inch	Mathematics	Music
Educational Theory		0·80	0·79	0·42	0·64	0 40	0·09	0·08
History	0·80		0·71	0 71	0·38	0 46	0·31	0·25
English Literature	0·79	0·71		0·66	0·58	0 30	0·28	0·00
Geology	0·42	0·71	0·66		0·24	0·41	0 45	0·30
Hygiene	0 64	0 38	0·58	0·24		0·47	0 29	0·02
Weight per Inch	0·40	0·46	0·30	0 41	0·47		·08	0·44
Mathematics	0·09	0·31	0·28	0 45	0·29	– 08		0·14
Music	0·08	0·25	0·00	0 30	0·02	0·44	0·14	

The laws which are determined by this method of correlation are only true "on the average" as we say, in large numbers of cases. But this as a matter of fact is also the case with the most exact laws of physics, as Clerk Maxwell long ago pointed out in the case of the laws of thermodynamics. In biology, psychology, education, economics, and the like the individual is easily observed and his importance not likely to be overlooked. In physics the individual atom or individual electron is not so easily tracked, though I am told that some recent work succeeds in following the flight of individual electrons. But even when we examine the individual in say psychology we find that he himself is "statistical," is influenced by countless forces, and that his most individual acts, as say, making a personal decision, are of the nature of striking an average: and I should be inclined to suspect that even the individual electron would be found to be complex, or at any rate its individuality and its individual behaviour when analytically studied be found to depend on the constancy of the average of large numbers of small factors.

It will be seen that I consider that all scientific laws are in fact statistical: for if we go behind any statistical enquiry to examine the individuals about whom it is concerned we merely destroy one individuality to create a number of other individualities which are themselves in due time found to be statistical in nature and to demand further analysis, and to this process I cannot see that there need be any end. I suppose it is from the desire of keeping under continued observation one individuality as such that the need of a synoptic view arises, about which Dr. Merz in recent papers has spoken. But even though the process be endless it is nevertheless the problem of science to carry it out; and it happens that in heredity—from the study of which I have taken my examples—a step behind the statistical view which has been considered in this paper has already been taken.

The Galton School, using the methods of correlation, is studying the average. The Mendel School, on the other hand has come upon a law dealing with comparative units of inheritance. Perhaps as our knowledge of Mendelian units increases, it may become possible to explain by their random combination the average results which are being measured by the biometricians, much as the laws of mechanics applied to individual atoms explain the properties of matter in bulk.

CONCLUSION.

Plurality of Causes is probably, and Plurality of Effects is almost certainly, only apparent and is due to our inability to analyse: but as to analysis there appears to be no finite end, so this plurality never can disappear, and the Methods of Agreement and of Difference, and the Joint Method seem to be applicable strictly speaking to abstractions only, never to actual things. To these the Method of Agreement and Disagreement, a wider and more definite form of Mill's Empirical Method (which it includes) has to be applied and in its quantitative form this becomes the Method of Correlation. The Method of Agreement and Disagreement includes all the other qualitative methods as special cases and corresponding to these on the quantitative side are different problems in the Theory of Correlation on which much mathematical labour has been expended during the past ten years, leading to important extensions of the Theory of Probability. The result of this has been the publication of formulae and tables which make the testing of inductions a mechanical proceeding. The inductive leap itself is of course not mechanical, yet the readiness with which suspected causation can be tested is a great incentive, and the process of testing often suggests new ideas. The new devices are a distinct advance and are the weapons by which induction is conquering new and difficult regions, and as such they will doubtless appear in good time in the text books of logic.

DEFORESTATION IN ANCIENT GREECE.

By MAURICE S THOMPSON, M.A

[Read May 15th, 1913]

In considering the conditions under which ancient Greek civilisation rose and declined, one of the most important questions to be decided concerns the actual state or even appearance of the land itself.' Was ancient Greece as poor a country as Greece is at the present day or were the hills that are now barren then covered with soil and trees, and the land as a whole in consequence fertile instead of being, as it is now, one of the poorest on the northern shores of the Mediterranean sea? And if ancient Greece was fertile and well forested while modern Greece is not, when and why did the change occur? Was it gradual or rapid and can it be shown in any way to have affected the development of civilisation? That some change has taken place is apparently usually admitted, but the difference between past and present is either considered to be slight or of little consequence; at least in most discussions of ancient Greek economic conditions it is almost completely ignored. The present paper makes no pretence of attempting a definite or conclusive answer to the questions propounded above; at best it offers a few suggestions on a somewhat obscure and neglected subject.

It is perhaps necessary at the outset to realise what the present condition of the country is, especially as in many accounts its natural poverty seems often underestimated. The great variety in crops and vegetation possible in any mountainous land in a warm climate is especially marked in the case of Greece where plains may vary in elevation as much as 2,000 feet, and arable land even to a greater extent. This variety at first sight is liable to be mistaken for abundance and wealth, which is a very different thing.

For example, a village in Aetolia may produce within its small territory, tobacco, vines, olives, figs and corn, and of corn perhaps have two harvests with a six weeks interval between them and yet at the same time be poverty-stricken and continually short of food supplies. The poverty of the land to-day is far from being entirely due to lack of development and bad farming, though much could be done to improve it According to statistics 64 per cent of the whole country is barren, that is to say at best it grows scattered tufts of a prickly scrub which affords sustenance for goats. About 8 per cent. is occupied by pasture, about 18 per cent. is under the plough though part of this area is almost more stones than soil. The remaining 9 per cent. is nominally under forest, but only a portion of this grows trees of sufficient size to be of use for timber, and even where the trees are large enough the great difficulties of transport in the hills renders them largely useless.[1] The general lack of large and useful timber is seen in a variety of ways: the use of large scaffolding in building is carefully avoided; sheds are commonly of sundried mudbrick except in a few favoured localities; the small wooden pegs used by the cobblers to sole boots instead of nails are frequently imported from Austria owing to the lack of hard wood of any kind

What the country needs at present to make it fertile is soil and trees on the hills and mountains, neither of which is possible without the other. Trees on the hills might perhaps increase the rainfall, but they would without doubt hold up the soil on the slopes, check denudation and prevent the winter rains pouring off the land into the sea before the soil can benefit.

No one who is acquainted with the northern part of Pindus where the forests still survive can have failed to notice how closely deforestation is followed by the denudation of the hills. Some of the villages in that district just to the north of the Thessalian frontier subsist very largely on trading in timber. The inhabitants of one of these began to cut the woods on the mountain slopes directly above the village. In a very short time the side of the hill began to slip away, streams that had hitherto flowed harmless down the village street increased in size; one cut out for itself a large ravine and swept away a group of houses. The damage was sufficiently great and the cause sufficiently obvious to induce the village to act on its own initiative. Wood-cutting above the village was stopped, replanting was tried and goats and sheep kept out. By these measures the immediate damage has been checked, but the ravine is still gradually increasing in size. Unless the village had fortunately happened to be in the way, the wood-cutting would have continued, and there is hardly the slightest doubt that by this time the whole hillside would be bare. Deforestation in the hills--in the plains it is a different matter—is in Greece followed by denudation in a very few years. Then as soon as the hills are bare of soil the plains in their turn suffer by the rush of water from the hills. There is no place in Greece excluding a small area in Thessaly which is as much as 10 miles from a mountain, and a characteristic of Greek mountains is their steep lower slopes and general lack of foot hills, features which help to increase the rush of waters on to the plains.

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[1] Aristotle's ideal city had to be easily accessible for the carrying of timber. *Pol.* IV., 5.

Information on the question of deforestation and denudation in the past is from the very nature of the case hard to acquire. The evidence such as it is may conveniently be divided into archaeological, literary and geological, and though in many respects it is admittedly slight, nevertheless it seems to show first that there once was a time when Greece was a well forested and fertile land, and secondly that the effects of deforestation and denudation had become a serious economic question by the beginning of the fourth century, B.C.

In the prehistoric age northern Greece, and Thessaly in particular, was inhabited up to the end by a people who were still in a low state of civilization. The northern part of the country in fact never came under the full domination of Mycenean civilization so prominent and extensive in the south. In Thessaly either in the plains or at the edge of the hills over 50 settlements of these primitive people are now known, but all these sites are to the east of an imaginary line drawn north and south through the modern town of Kardhitsa and in the plains west of this line no early remains have yet been discovered. This cessation of human habitation in the plains about 10-15 miles short of the range of Pindus which forms a natural boundary requires some explanation and the simplest solution of the problem seems to be that we have here the limits of a forest belt that has long since disappeared.[2]

In south and central Greece where Mycenean civilization flourished in the prehistoric period, the distribution of sites is not nearly so clearly defined; nevertheless it is noticeable that the Mycenean sites as a whole cease at a lower level and keep nearer to the plains than the later Hellenic sites and that these in turn cease at a lower elevation than the villages of a still later period Thus as time went on the sites of habitation have extended upwards In the earliest times there seems to have been an objection to living in the hills, and in the case of Thessaly to living in a certain part of the plains, and this objection seems gradually in course of time to have been removed. Here again it seems probable that the extension of the inhabited area followed the retreating forest line. To this generalisation that applies to the mainland especially, the island of Crete is an exception. Early sites in Crete are found at a considerable elevation; but since Crete was civilised centuries before Greece itself it is only natural to suppose that the forests would decrease there at a correspondingly earlier period. The exceptional position of Crete helps to confirm the rule.

Apart from their distribution the remains of the Mycenean age are evidence in another way. The reconstruction for example of any of the great Mycenean palaces would at the present day involve the importation of timber. Beams of a sufficiently large size could hardly be procured in the country. They might perhaps be found in northern Pindus, but even so for purposes of transport would have to be hewn into small logs or planks. The Mycenean palace assumes a better supply of timber locally than now exists. Passing from the remains of the Mycenean age to those of the historical period we are led to a similar conclusion. The architecture of a Greek temple is clearly derived from a wooden prototype even in its minute details. Thus the row of guttae are the wooden pegs converted into marble or stone, and the external painting of the marble temple probably goes back in origin to the wooden originals. That not only the prototype of the Greek temple was of wood, but that wooden temples once existed in Greece is beyond question. The early temple at Thermon was largely of wood but the best example is the temple of Hera at Olympia a building of a large size which though later of stone in its earlier stages was of wood. When excavated it was noticed that its columns were placed at an unusually large distance apart, and that no two were exactly alike or seemed to be of the same period nor was any trace of stone work above the columns found at all Pausanias[3] in his account of this temple records that in his time one column was of wood. This confirms what might otherwise have been assumed from the differences between the stone columns and the large space between each, and the theory is usually accepted that this temple was originally of wood and that as the wooden columns decayed they were replaced by stone. The last wooden survivor seen by Pausanias was as might be expected in the interior and so not exposed to the weather. The question of interest is why did the Greeks in this most inartistic way patch a wooden building with stone? I suspect the answer is that beams of sufficient strength and 14 feet in length, which is the height of the columns, could not

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[2] *Prehistoric Thessaly*, Wace and Thompson, p 6, Cambridge, 1912.

Paus. v., 16.

easily be procured. Timber of a large size was difficult to obtain. The few Byzantine and Mediaeval buildings that survive in Greece show an economy of woodwork greater perhaps than the buildings in the north of the peninsula.

In using literary evidence for the appearance of ancient Greece in one respect in particular considerable caution is needed. Descriptions of scenery which are for our present purpose most valuable are apt at times to be most misleading, for the particular place described may not be typical of the country and in many cases very probably is not. To illustrate this by what I strongly suspect is an example. The one place in Greece which appealed as scenery to the ancients was the vale of Tempe,[4] and yet as a rocky gorge it is inferior to many in Greece. It so happens, however, that Tempe is the one place in Greece to-day that appeals strongly as beautiful to the ordinary Greek peasant, and the attraction at the present day is admittedly due to the fact that there can be seen there even in summer green grass, large trees and a river with water in it. In short, Tempe is liked to-day because it is typically not Greek. Whether this was the case or not in ancient times, it is impossible to say; but it is quite probable it was. At all events descriptions of scenery are liable to be misleading. The numerous epithets and similes referring to mountain woodland in the Homeric poems suggest at least to myself a fertile and wooded Greece.[5] Flocks and herds are common in Homer and Homeric food supplies are a striking contrast to the meagre vegetarian diet of Plato's Republic. This difference is of some importance as it is not solely explicable by the increase of agriculture at the expense of pasture land, since the two in Greece except in the case of the fields left fallow hardly overlap. To banish agriculture from Greece to-day would only mean an increase of winter pasture, an increase of summer pasture would require soil on the hills. One passage in the Iliad by itself is almost decisive.

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[4] Pliny, *Nat. Hist.*, IV., 8. *Cf.* Livy, XLIV, 6. Herodotus' interest is geological.

[5] It seems useless to give a list of instances, which, taken singly, are of little weight. *Cf.* Browne, *Homeric Study*, p. 125, and Geddes, *Problem of the Homeric Poems*, p. 250.

> "At the time when the woodman weary of felling tall trees makes ready his meal then did the Achaeans charge through the ranks . . ."[*]

If it be assumed, as apparently it usually is, that these lines were intelligible to the Homeric audience, and that they knew what time of day was intended, it follows that Homeric Greece was far and away more wooded than Greece is at present.

In contrast we may turn to the account of Attica at the opening of the Critias; it is a comparison between what Attica was in Plato's own day, the end of the fifth and beginning of the fourth century, and as he supposed it to have been at the time of the mythical war of Attica and Atlantis which happened 9,000 years before his time.

"The consequence is that, in comparison of what was then, there are remaining only the bones of the wasted body, as they may be called: in the case of the small islands all the richer and softer parts of the soil have fallen away and the mere skeleton of the land is left. But in its primitive state the mountains were high hills covered with soil and the plains were full of rich earth, and there was abundance of rich wood in the mountains. Of this the last traces still remain, for although some of the mountains are now only sustenance for bees, not so very long ago there were still to be seen roofs of timber cut from trees growing there, which were of sufficient size to cover the largest house: and there were also other high trees cultivated by man and bearing abundance of food for cattle. Moreover, the land then reaped the benefit of the annual rainfall; not as now losing the water which flows off the bare earth into the sea, but having an abundant supply in all places and receiving it into itself and treasuring it up in the loose clay soil, it let off into hollows the streams which it absorbed from the heights providing everywhere abundant fountains and rivers: of these sacred memorials may still be observed, in places where fountains once existed: and this proves the truth of what I am saying."[7]

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[*] *Iliad* xi., 86. The usual text reads 'δεῖπνον' but Zenodotus read 'δόρπον'—the time of this meal seems to have troubled the commentators.

The main points of interest in Plato's account of Attica are these: that the deforestation and denudation are directly and rightly connected and that both are thought to be recent, the remnants of the old forests are in fact remembered. From this passage we may therefore conclude that by the time of the 5th century deforestation had become a serious question in Attica. This date may perhaps be confirmed by certain archaeological evidence. The Pentelic marble quarries were not worked until the 5th century; were they only discovered after the trees had disappeared? The account in the Critias excepting the reference in it to still greater denudation in the islands only refers to Attica, and there are several reasons for suspecting that Attica was more liable to suffer from deforestation than other parts of Greece. Acharnae was a well-known centre of charcoal burning, a most destructive trade and the silver mines of Laureion must have been a severe drain on any local supply of timber. The evidence therefore for Attica cannot be taken as applying equally to the rest of Greece.

The Homeric Hymn to Apollo, which unfortunately cannot be accurately dated, preserves the tradition that forests once covered the Theban plain, though when the poem was written they had ceased to exist.[8] At the time of the siege of Plataea in the latter part of the 5th century we hear of timber being cut on Cithaeron[9] which still possesses some trees. There is reason to suppose, however, that even in the 5th century this supply was strictly limited or of a poor quality; for Theopompus describes how during the Peloponnesian war the Boeotians acquired wealth by carry ing off wooden beams from the abandoned houses in Attica

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[7] *Critias*, III, Jowett Trans. [8] Hom Hymns III., 227 [9] Thuc. II., 75.

This story has further point when it is remembered that the inhabitants of Attica on leaving their houses and retiring into Athens took with them all the wood work they could.[10] Parnassus, which in Homer is forest clad, like Cithaeron still possesses some woods, but by the fourth century the timber from there was considered among the worst in Greece.[11] Among places where trees existed in antiquity but which now are completely bare are the plain between Tegea and Mantinaea,[12] and the region round Thaumaki.[13] From the fifth century onwards timber for shipbuilding was difficult to find within Greece, and the best came from Macedonia, Thrace and Pontus. Thucydides notes that the loss of Amphipolis deprived the Athenians of a source for timber, and the dependence of the Greeks on Macedonian timber in the fourth and third centuries is noticed frequently.[14]

An inscription found at Olynthus of a treaty between Amyntas III. and the Chalcidians[15] regulating the import of timber and especially of pine illustrates the great importance of the Macedonian forests. Theophrastus, who gives a list and some account of the best places for ship timber, places Macedonia first, Pontus second, Rhyndacus third; then comes the timber from Greece, of which that from Parnassus and Eubœa was the worst, the Arcadian timber slightly better and that from the Aenianes the best. The quality of the wood used by the Greeks for shipbuilding was probably not high. The ancient Greek ships were not meant to face a storm, under normal circumstances they never put to sea in winter, and even so they had constantly to be repaired. Even the Athenians at the height of their naval supremacy could not supply their troops at Pylos throughout the winter months.[16]

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[10] Thuc II., 14.

[11] Theophrastus, *Hist Plant.* IV., 5, 5; V., 2, 1. [12] Paus. VIII., 11.

[13] Livy XXXVI, 14. The Laconian Asine seems once to have had forests Leake, *Morea* I., p 435. Thuc IV., 13.

[14] Thuc. IV, 108. Xen V., 11, 16. Diod. XX., 46. Plutarch, *Demetr*, 10. Demos XLIX., 26; XIX, 265 *Cf* also Hdt V, 23. This list is by no means complete

[15] Dittenberger Sylloge^2 No. 77*b* = Hicks, *Hist. Greek Inscrip.*, No. 74

Not only timber for ships had been obtained outside Greece: the Eleusinian inventory records payments made to sawyers of Macedonian timber, and refers to squared logs from the same locality.[17]

Details of the scaffolding used for the Parthenon and Propylæa are not known except that pains were taken to sell it off when done with and this alone suggests that wood for scaffolding was rare.

In a certain number of cases there is definite evidence for changes in the coast line having occurred within historical times, and other geological changes within the same period seem also to have happened in some of the valleys and plains. How far any of the examples given below may be due to the denudation of the hills must be left for geologists to decide. It is unfortunate that Greece as yet from the geological standpoint has not been fully examined.

Since the days of the battle of Thermopylæ the silt brought down by the Spercheus has added about 30 square miles of land to the coast of Malis, and Thermopylæ as a narrow pass has now ceased to exist. It would be interesting to know whether the rate of increase was as rapid before the fifth century as it has been since. The existence of prehistoric settlements which date from about 2,000 B.C. and probably were never on the coast, near to the village of Amuri, not far from Lianokladhi, may perhaps help to solve this question.

The rapidity with which the coast line in south Acarnania was increasing during the fifth century owing to the silt washed down by the Achelous was noticed both by Herodotus and Thucydides.[18] In both these cases denudation of the interior seems to be implied.

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[16] Thuc. iv., 27.

Greek navigation is generally vastly over-rated It is possible to go so far by coasting voyages in the Aegean that the open sea was not attempted The incentive of deep sea fishing is also lacking .

[17] Dittenberger, *op. cit*, 587

[18] Hdt. ii., 10. Thuc ii, 102.

In Phocis in the Cephissus valley which leads into lake Copais some change may be suspected The classical sites are at the sides of the valley on the bottom slopes of the hills but by Chaeronea there is an early pre historic site in the centre of the valley in a position now often flooded out in winter In the pre historic age the waters of Lake Copais used to flow out into the sea opposite Euboea through a number of natural tunnels This natural system of drainage was not entirely satisfactory for before the end of pre historic age the waters of the lake were controlled by a system of stone dykes At a later period the tunnels became blocked and inefficient and the dykes in consequence became useless or else were neglected The precise effect on the area of the lake except that there were floods is obscure but an attempt was made to drain it in the fourth century by Crates a mining engineer from Chalcis[19] Thessaly also provides an example of lakes having changed their size In Strabo s time Boebeis was much smaller than Nessonis At present the reverse is the case The small island of Calauria since classical times has been joined to the next isle by a bank of mud[20]

To sum up in conclusion the various suggestions made above In the first place it is extremely hard if not impossible to imagine any great civilisation developing in a land as barren and unfertile as Greece is now There is an *a priori* case for some change having occurred From certain archaeological evidence moreover it appeared that though in early times Greece seems to have been well forested the supply of timber later began to fail The literary evidence on the question also points to a similar conclusion The Homeric poems suggest a fertile and well treed country the accounts of the fifth and fourth centuries indicate clearly the opposite the old forests are still dimly remembered, but the land in parts at any rate is already suffering severely from lack of woods and denudation, and for large timber is dependent on Macedonia and Thrace. The Greece of Plato and Aristotle is in fact not so very far removed from the Greece of to-day Notice, for example, the food supplies, and the regulations about water. Lack of food is assumed by Plato to be a normal cause for sending out a colony. The idea of a self-supporting town has become nearly incredible; for though the city in the Politics is said to be "self-sufficing," yet like the city in the "Laws," it has "a little of most things but not abundance of any." Dependence on foreign supplies, though in theory detested by most Greek philosophers, is admitted in their Eutopias. The mention of forest guardians among the officials in the Politics is by itself significant: forests are never looked after till they have mostly disappeared.[21]

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[19] Strabo 407 It is now drained by canals

[20] Paus II 33 *Cf* Leake *Morea* II p 450 I have seen it stated but cannot recall where that a shoal by Hydra seems to have been formed by the denudation of that island

The date suggested for the time by which denudation and deforestation had become a serious question, corresponds with the beginning of the decline of Greece and although political failures were the chief causes, the increasing poverty of the land itself may have had some effect. Some economic reason seems to be needed to explain why the Greek cities outside Greece continued to prosper after those in Greece itself had almost ceased to be of any importance. It also happens that Thessaly which became important late, continued to be so after the rest of Greece, and in the fourth century Thessaly alone of the Greek states was exporting corn.[22] Now Thessaly is a region that can only suffer from denudation to a very limited degree; it consists of a plain surrounded by hills through which there is only one outlet, the valley of the Peneus. The Peneus also does not drain all the plains. Thus the soil of Thessaly is safe for all time.

As to why the forests perished various reasons may be given. It has several times been suggested that within historical times the climate of Asia has become considerably drier and a similar change may have occurred in the Aegean as well.[23] Unfortunately there is insufficient historical evidence to test such a theory thoroughly for the region where it has been mainly applied. Moreover, in connecting a climatic change of this kind with deforestation, the question at once arises as to which is cause, and which effect. The destructive capability of mankind seems by far the most probable solution. Reckless timber cutting without replanting, and replanting by itself is of little use in a goat country; the increasing demand for wood for shipbuilding and above all for mining; forest fires casual or otherwise and the destruction of woods for military purposes[24] have all to be considered. Outside Greece there is ample evidence for rapid deforestation on a large scale by human agency; for example as late as the 16th century many of the hills of Castile were under forest Lastly the fact that the most thickly wooded parts of Greece are those which in the past were most thinly populated argues that man has been the main cause rather than climatic change.

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[21] *Cf* in particular, *Politics*, IV , 5 and 12 *Laws*, VIII., 704, 705, 804.

[22] Xen , *Hell.*, VI , 1, 11.

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[23] Some suggestions on this point may be found in *The Mediterranean Pilot*.

[24] Xerxes destroyed woods in Macedonia, Hdt. VII., 131. The wood at Pylos was fired by mistake, Thuc. IV., 30.

COLOUR CHANGES IN COLLOIDAL GOLD.

By S H LONG, B Sc.

[Read May 19th, 1913.]

INTRODUCTION.

The existence of two gold colloidal solutions has long been known One of these is red and the other blue. The distinction between these two colloids has been examined by Lampa.[1] This investigator centrifugalised various solutions of colloidal gold and from time to time investigated their absorption spectra by means of a spectral photometer. He found that the transparency of the solutions towards red increased in much greater proportion than the transparency towards blue during the time the solutions were centrifugalised Since the process of centrifugalisation brings about the deposition of larger particles, he concluded that in red colloidal gold solutions the particles were smaller than in blue solutions.

The object of this paper is to study colloidal gold solutions further, particularly from the point of view of colour.

EXPERIMENTAL WORK.

Colloidal gold solutions in water were prepared by the new high frequency arc method,[2] this method is particularly suitable for the preparation of colloidal solutions as it lends itself to a great variation of conditions, such as a change of voltage, amperage, frequency or arc-length of the arc burning under the solutions in which the colloids are prepared. By variation of the electrical conditions three colloids of gold could be prepared. These were respectively red, blue and purple in colour. The one most commonly prepared was purple. These solutions were then subjected to 100 volts in a Hardy tube. This piece of apparatus consists of a large U tube which is partly filled with distilled water. Platinum electrodes dip into the tops of the arms of the U tube. By means of a third tube fitted with a glass tap and sealed into the bend of the U tube the colloidal solutions were allowed to flow into the apparatus rising to an equal height in either arm of the U tube. If the colloids were allowed to flow in slowly and carefully, one obtained a quantity of colloid in the bend of the tube separating two columns of distilled water which stretched to the platinum electrodes. The surface of separation of the distilled water and colloid was distinctly visible, and even when motion of the colloid took place, this surface of separation persisted.

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[1] *Akad. Wiss. Wien. Sitz. Ber.*

[2] *Proc. Univ. Durham Phil. Soc.*, 1913, v., p. 68.

When the colloids were placed in the Hardy tube and subjected to 100 volts, the following results were observed:—

(1) The blood red colloid of gold moved wholly to the Kathode in about 6 minutes.

In allowing this action to continue for about 15 minutes there was a gradual transference of the solution from the Kathode arm to the Anode arm accompanied by a change in colour from red to purple and finally to blue. When this action was allowed to continue for 60 minutes it was noticed that all the solution had changed from red to blue, and from the Kathode arm to the Anode arm.

(2) The deep blue colloid of gold moved wholly to the Anode, and although it was allowed to stand under the action of 100 volts for 120 minutes no further changes were observable.

(3) The purple solutions of colloidal gold had a double action showing them to be really a mixture of the red and blue colloids. The purple colloids divided into red solutions which ascended the Kathode arm and blue solutions which ascended the Anode arm.

On allowing the action to continue for some 30 minutes, it was noticed that the Kathode tube became more feeble in colour while the colour in the Anode tube became a denser blue After 60 minutes the Kathode arm of the Hardy tube was practically colourless; while the Anode arm was a much deeper blue colour. Although the action was allowed to continue for 120 minutes, no further changes were observed.

A quantity of red colloidal gold was taken, and into it a very minute quantity of common salt was placed. This caused a gradual change in the colour of the colloid from red to blue.

These results are in agreement with those of Blake.[3]

By a variation of electrical conditions the following colloidal solutions were prepared and placed in Jena glass flasks to minimise any action occurring between the colloid and the glass as might have been the case if ordinary flasks had been used. After standing for some three months the following results were obtained.

Colour immediately on preparation	Colour after standing 3 days.	Colour after standing 3 months
Faint Reddish Purple	Bluish Purple	Blue and no reddish colour visible.
Red	Reddish Purple ...	Blue.
Reddish Purple .	A strong Bluish Purple	A Deep Blue.
Very Deep Blood Red	Bluish Purple .	A very intense Blue.

Here again the change is probably due to the presence of an electrolyte which dissolves out of the Jena glass.

THEORY OF EXPERIMENTAL RESULTS.

The explanation of these results is based on facts put forward by

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[3] *Amer. Journ Science*, 16, pp 433-441, Dec., 1903.

Helmholtz (*Wied. Annalen VII.*, p. 337, 1879. Memoirs Lond. Phys. Soc.),
Lamb (*Brit Assoc. Rep.*, 1887, p. 495),
Noyes (*Journ. Amer. Chem. Soc.*, vol. xxvii., No. 2, p. 85),
Burton (*Phil. Mag.*, April, 1906, p. 425).

Whitney and Blake (*Jour. Amer. Chem. Soc.*, xxvi, 1339, 1904), after working on the movement of colloidal particles in an electric field concluded that the direction of motion of the colloids depended upon the associated ions.

The colloidal gold solutions used in this work were prepared in water. In this liquid we have two kinds of ions present, namely $(OH)^-$ ions and $(H)^+$ ions: Thus we ought to be able to form a double series of colloids of gold in water one in which we have charged gold particles possessing a negative charge and associated with $(H)^+$ ions and the other in which we have positively charged gold particles associated with $(OH)^-$ ions.

From the experimental results obtained, the following conclusions were drawn:—

(1) Red colloidal gold in water is associated with the $(OH)^-$ ions and moves to the Kathode. In this solution the particles are very small.

This colloid may be represented by the formula

$$n.Au^+ + H^+OH^- = Au_n^+(OH)^- + (H)^+$$

We may consider the above thus. Depending upon the conditions under which the gold was dispersed under the water, a group of particles received a positive charge and on entering the disperse medium became associated with the negatively charged (OH) ions forming a gold-hydroxyl aggregate. This by a slight displacement may be looked upon as a group of gold particles surrounded by an atmosphere of hydroxyl ions.

(2) Blue colloidal gold in water is associated with

the (H) ions and moves to the anode In this solution the particles are larger than in the case of red colloidal gold.

It may be represented by the formula

$$n.Au^- + H^+OH^- = Au_n^-(H)^+ + (OH)^-$$

Again in this case, we may consider that according to the conditions of dispersion, the gold particles have received a negative charge. On entering the disperse medium they have become associated with the $(H)^+$ ions forming the aggregate $Au_n^-(H)^+$ which on slight displacement may be considered as a group of charged gold particles, negatively charged, and surrounded by an atmosphere of positively charged hydrogen ions. The number of attached ions to the aggregate will depend upon the charge possessed by the group of gold particles.

(3) Purple colloidal gold is simply a mixture of the red and blue colloids, and the different shades of purple depend upon the proportion of red to blue present in the purple solution.

(4) The more stable form of gold colloid is the blue, and the red may be changed to blue by two actions, namely:—

(1) Action of an electrolyte.
(2) Action of an electric field

Further experiments were done on this work as follows: A blood red solution of colloidal gold was placed in a cell as used in work on absorption spectra. Into this cell two platinum electrodes dipped and these were connected to the 100 volts supply. The absorption spectra were photographed every two minutes, each exposure being of 30 seconds duration. In this way 9 spectra were obtained. During the time these spectra were photographed it was observed that the colour of the colloid changed from blood red to a deep blue. This colour change set in after the colloid had been subject to 100 volts for about 5 minutes. The action was allowed to continue for 60 minutes. At the end of this time the colloid was precipitated as a deposit of very fine dark particles. No further colour changes were, however, observable.

The spectra showed the following results:—

While the colloid was red the spectra showed a large absorption band extending throughout the orange region. As the colour changed to purple this band gradually narrowed down and the edges became more sharply defined. When the colloid was blue in colour the absorption band was only about one-sixth as long as when the colloid was in the red stage and the edges were very sharply defined in the blue condition.

SUMMARY.

(1) Red colloidal gold is an aggregate consisting of positively charged particles surrounded by hydroxyl ions, these latter being slightly displaced so as to form an atmosphere about the former.

(2) Blue colloidal gold may be considered to be an aggregate of negatively charged particles surrounded by positively charged hydrogen ions, these being slightly displaced so as to form an atmosphere about the charged particles.

(3) Purple collodial gold is a mixture of the two previously mentioned colloids, and the various tints of purple depend upon the amounts of red or blue present.

(4) The more stable form of gold colloid is the blue, and the red may be changed to the blue by the action of (1) Electrolyte (2) Electric field.

In conclusion, I wish to thank Professor Stroud for his kindly interest in this work.

My best thanks are due to Mr. H. Morris-Airey for suggesting the original work on colloids, from which the present paper was one development.

A Method of Correcting the Colour Sensitiveness of a Photographic Plate used in Spectroscopy.

By H Morris-Airey, M Sc., and S. H. Long, B Sc.

[Read May 19th, 1913]

The sensitiveness of a photographic plate for light of different wave-lengths is in general not the same, but shows a marked selectivity for certain regions of the spectrum. An "ordinary" plate usually has its maximum sensitiveness at about 4,500 Å, and the total range of the spectrum for which it is sensitive does not extend more than 500 Å on either side of this. By bathing the plates in various dyes, which in general have a reddish colour, the range of sensibility can be extended to cover the range from 3,000 Å to 7,000 Å. During this range the sensitiveness is, however, far from uniform, and may have very pronounced maxima and minima. The result of this is that the spectrum of a continuous source often shows a banded appearance like an absorption spectrum. The application of the Bunsen Roscoe law then leads to an erroneous estimate of the distribution of the energy in the spectrum of the radiating body. In photographing line emission spectra this defect is not a serious one, but it may become very disturbing in the case of absorption spectra. The irregularities of the sensibility can be smoothed out to some extent by using suitable filters such as Aesculine, Picric acid, etc., but none of these compensate the plates sufficiently for absorption spectra work. A more successful method of correcting the sensibility of the plate, which we have recently used, is to place in front of the plate a screen whose transparency is graded so as to reduce the intensity of the light at those parts of the plate where the most active wave-lengths are falling.

Such a screen can be made photographically, by exposing a plate of the same make as the one to be corrected, to the spectrum of the light which is used for the source in the photography of the absorption spectrum. The negative thus produced does not show a uniform dark band, but where the least active wave-lengths have fallen there is little or no blackening. Thus the negative presents a series of light and dark bands, the dark bands being produced where the most active wave-lengths have fallen, and the light bands where the least active wave-lengths have fallen. When this negative is used as a screen in contact with another photographic plate, which is exposed to the spectrum of the source, the more active wave-lengths are weakened whilst the less active ones are practically unaffected. In this way it is possible, by suitably adjusting the exposures of the screen plate and finished plate, to obtain a spectrum of uniform blackening on the finished plate, the light and dark bands being smoothed out by the screen plate and the colour sensibility of the finished plate is thus adjusted to one degree sensitiveness.

PAPERS READ BEFORE THE SOCIETY,

SESSION 1912-1913.

GENERAL MEETINGS.

December 6th, 1912 "Notes on the Third International Archaeological Congress held at Rome in October, 1912," by J. Wight Duff, M.A., D.Litt.

May 2nd, 1913 "The Structure of Metals,"* by Sir J. Alfred Ewing, K.C.B., F.R.S.

May 15th, 1913 "Deforestation in Ancient Greece,"* by M. S Thompson, M.A.

SECTION A.

November 21st, 1912 "The Basic Properties of Sulphoxides," by A. Forster, M.Sc., Ph.D. "The Time-Average Value of Uranium and its Connection with Geological Time Measurement,"* by R. W. Lawson, B.Sc

December 9th, 1912 "Ionisation in Gaseous Mixtures by Rontgen Radiation,"* by L. Simons, B.Sc.

January 30th, 1913 "A Note on the Velocity of Light and Group Velocity," by T. H. Havelock, M.A., D Sc. "Abnormal Milk from Cows that have been in Milk for a long Period," by S. H. Collins, M.Sc. "Burnt Bones, a waste Product of Steel Finishing," by S. H. Collins, M Sc.

March 13th, 1913 "Note on a New Electrical Method of Preparing Aqueous Colloidal Solutions of Gold,"* by H. Morris Airey, M.Sc., and S. H. Long, B.Sc.

May 19th, 1913 "Benzyltetrasulphoxide," by J. A Smythe, D.Sc., Ph.D. "A Method of Correcting the Colour Sensitiveness of a Photographic Plate used in Spectroscopy,"* by H. Morris Airey, M.Sc., and S. H. Long, B.Sc. "Colour Changes in Colloidal Gold,"* by S. H. Long, B.Sc.

SECTION B.

January 9th, 1913 "The Great Whin Sill at Kirkwhelpington,"* by G. Weyman, B.Sc. "Two Recently Discovered Whin Dykes on the Northumberland Coast," by J. A. Smythe, D.Sc., Ph.D.

February 11th, 1913 "The Geology of the Cleadon Hills," by D. Woolacott, D.Sc., F.G.S. "The Geology of the Tyne," by E. Merrick, B.Sc.

February 25th, 1913: "A New Factor in Diet," by F. A. Bainbridge, M.A., M.D., D.Sc.

* Indicates Papers published in the *Proceedings*.

SECTION C.

March 14th, 1913. "Minor Planets," by F. C. H. Carpenter, M.A.

SECTION D.

January 31st, 1913 "Norman Durham," by the Rev. H. Gee, D.D.

February 18th, 1913 "XVIIIth Century Men of Letters in the North Country," by J. Oxberry.

March 5th, 1913 "Excavations on the Site of the Roman Town at Corbridge, Northumberland," by W. H. Knowles, F.S A.

SECTION E.

November 14th, 1912. "The Influence of the Conductivity on the Apparent Dielectric Constants of Liquids,"* by W. M. Thornton, D.Sc., D.Eng.

January 28th, 1913. "An Experiment to Test Rayleigh's Theory of the Resistance of Alloys," by H. Morris Airey, M.Sc.

February 21st, 1913 "The Recent Rise in Prices," by H. Hallsworth, M.A.

SECTION F.

October 25th, 1912 "Personality," by F. B. Jevons, M.A., D.Litt.

February 6th, 1913 "On the Synoptic Aspect of Reality,"* by J. T. Merz, D.C.L., Ph D.

May 8th, 1913 "Mathematics and the Inductive Methods of Logic,"* by G. H. Thomson, D.Sc., Ph.D.

LIST OF MEMBERS OF THE SOCIETY.

SESSION 1912 1913.

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UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY.

ANNUAL REPORT OF COMMITTEE FOR 1912-1913.

During the Session 1912-13 four general meetings, one extraordinary general meeting and eighteen sectional meetings have been held.

At the General Meeting held on December 5th, 1912, the Duke of Northumberland was elected President. On May 2nd, 1913, the day previous to his installation as Chancellor of the University of Durham, the Duke of Northumberland presided over an Extraordinary General Meeting of the Society in Armstrong College, at which Sir J. Alfred Ewing, K.C B., F.R.S., lectured on "The Structure of Metals." The lecturer gave an account of recent work on the subject, much of which had been done by himself, and there was an audience of over 200. It is hoped that this will be the first of a series of lectures delivered before the Society by men of note.

The Society was represented at the British Association meeting in Birmingham by Dr G H. Thomson

The expectations expressed in last year's Annual Report as to the new Section (Philosophical Section) have been quite justified during the Session.

Three numbers of the Proceedings have been issued during the Session. The first number issued completed Volume IV.

The question of Exchanges for the Publications of the Society has been gone into and it is expected that the Exchange List will shortly be on a much more satisfactory basis.

The number of members at the end of Session 1912-13 was 166.

NOTES ON THE THIRD INTERNATIONAL ARCHAEOLOGICAL CONGRESS, HELD AT ROME IN OCTOBER, 1912.

By J. WIGHT DUFF, M.A., D.Litt.

[Read December 5, 1912]

Such literary unity as these "notes" possess may perhaps be compared with that of the early Latin *satura* which, as is well known, consisted of a medley of various contents. But inasmuch as no literary form was more characteristically Roman than this same *satura*, the present summary sketch may possibly appear to be not entirely inappropriate and not entirely lacking in "local colour." My aim, then, is to give an outline of the organisation and work of the Archaeological Congress in Rome; and to follow the account with lantern-slides, selected on what might be termed the true *satura* principle, to illustrate

(1) Some portions of Rome excavated in recent years;
(2) The first excursion of the Congress—to Cerveteri;
(3) The second excursion of the Congress—to Ostia;
(4) Certain valuable MSS. in the Vatican which I looked at myself with some attention.

This was the "Terzo Internazionale Archeologico Congresso," the two previous Congresses having been held at Athens, in 1904, and at Cairo, in 1908. The actual work of the Congress extended from the 9th to the 16th of October, 1912; but arrangements were made whereby *congressisti* could later in the month, under suitable guidance, visit Pompeii, Sicily, Sardinia and other parts of Italy. A subscription of 20 *lire* constituted one an effective member (*membro effettivo*) of the Congress, and entitled one to published transactions, which I understand there are but faint hopes of seeing complete. Enrolment conferred upon members considerable privileges, including reductions in railway fares not unwelcome to delegates who had to travel so far as I did; participation in receptions and excursions organised by the Central Committee; and—probably most valuable of all the right of free entry to all public museums and galleries throughout Italy during the whole month of October.

Apart from the intellectual stimulus given by the presentation and discussion of learned problems, one of the greatest attractions of such Congresses must always lie in the opportunity afforded of meeting men of European distinction. There were many eminent names; *e.g.*, from France, Cagnat, one of the very foremost epigraphists, with Toutain and Lafaye, both authorities on ancient religion; from Belgium, Cumont, who knows more than anyone else about the mystic ritual and symbolism of that fascinating Mithraism which spread from the east to our own local frontier of the old Roman Empire; from Germany, Dessau of Berlin, besides Schuchhardt, Thiersch and many others, including von Duhn of Heidelberg, whom I met in Crete years ago waiting for a chance steamer to take him back to Greece; from Greece itself, Lambros and Cavvadias were among the delegates; from our own country Evans, Waldstein, Percy Gardner and others In fact, nearly all the countries of Europe were represented, as well as several Universities of the United States and of the British Colonies. The various foreign schools of Archaeology in Rome sent their directors.

The Honorary Presidency was held by King Vittorio Emmanuele, and the Vice-Presidents were the Minister of Public Instruction and the Mayor of Rome. A large Committee in charge of the arrangements consisted of noblemen and senators, members of Parliament, and professors.

The wide field of labour to be surveyed was divided among twelve sections:—

(i) Prehistoric Archaeology.
(ii) Oriental Archaeology.
(iii) Prehellenic Archaeology.
(iv) Italian and Etruscan Archaeology.

(v) History of Classical Art.
(vi) Greek and Roman Antiquity.
(vii) Epigraphy and Papyrology.
(viii) Numismatics.
(ix) Mythology and History of Religions.
(x) Ancient Topography.
(xi) Christian Archaeology.
(xii) Organisation of Archaeological Work.

Two or more of these Sections frequently combined their sittings for the discussion of themes of common interest; for example, I attended several joint meetings, of vi and vii, the sections named *Antichità greche e romane* and *Epigrafia e papirologia*.

The place of meeting was usually at the *Università*, in the *Palazzo della Sapienza*, to give it the resounding Italian title; except that Section x, for Ancient Topography, generally met under the roof of Santa Francesca Romana, close to the Roman Forum, from which illustrative excursions were made to the Palatine; while the Section for Christian Archaeology paid visits of inspection to different catacombs.

Each Section had its own president, and often two secretaries. The official language of the Congress was Italian, and all announcements, oral, written or printed, were made in that language; but papers might be read and discussed in any one of the four languages, Italian, French, English, German.

On the day before the Congress opened, there were preliminary social gatherings; but the real inauguration took place, with much fitness, in the "Hall of the Horatii and Curiatii" on the Capitol at 10·30 a.m. on Wednesday, October 9th. In welcoming the delegates, the Mayor of Rome delivered a discourse marked by complete consciousness of the historic symbols of greatness around, but at the same time by an anxiety that the present should not be overlooked in excessive concentration upon the past. With this anxiety in his mind he was clearly insistent upon the fact, which archaeologists sometimes incline to ignore, that Rome is not a mere museum—"*Roma non è un museo,*" he said: "It is above all a modern city, the centre of a great country" (*sopratutto è una città moderna, centro di un grande paese*). He was followed by the Minister of Public Instruction, who was commissioned to speak for the King of Italy, and in his Majesty's name to declare the opening of the Congress. His speech appropriately called attention to matters of great moment for the domain of archaeology—to the recent advance made in excavations in the Roman Forum and on the Palatine; to the subsidies voted by the Italian Parliament proving the interest of the State in continued work at Ostia; and to investigations in progress or in prospect at many other places in Italy, such as the Licenza villa of Horace, and different sites in Umbria and Sardinia, and at Pesto, Pompeii and Taranto.

Among the speeches which followed the despatch of telegrams to the King of Italy and to Prince Constantine of Greece was the perfervid oration of Professor Lambros of Athens, who had been chosen to speak for the delegates. He paid homage to the magnificent historical and archaeological attractions of Rome, and made suitable allusion to contemporary difficulties with Turkey it was just before the Greek declaration of war when he remarked that in those anxious days the Greek delegates could enjoy at least a kind of "Olympic peace" in reflecting upon the past of their native land. It was a rhetorical effort, and was received by what Italian reporters called *applausi immensi.*

Thereafter, the work of the Sections began, most of them holding both morning and afternoon sittings, varied with late afternoon receptions arranged by the Ladies' Committee, the Agricultural Institute, the British School, and with very imposing evening receptions given by the Minister for Foreign Affairs at the Consultà on the Quirinal; by a lady archaeologist of note, the Contessa Lovatelli, in her *palazzo*; and again at the close of the Congress by the Municipality of Rome The two all-day excursions during the Congress were to Cerveteri in Etruria and to Ostia. These can be best illustrated with the aid of slides at the close of this paper.

Meanwhile, it remains to convey some slight notion of the range of topics considered at the Congress. It can only be a slight notion; for without devoting a large amount of space to the papers and their results, it would be impossible to give any just estimate of the work done. It must suffice to observe that the papers ranged over the whole field of archaeology, in the broadest sense, from prehistoric to medieval times; and the resultant variety of subjects will be readily gauged from some titles selected from the different groups and here given, for the sake of uniformity, in English:—

Prehistoric civilisation in Sardinia; Palaeolithic man in the Tiber-valley; Prehistoric implements from the Fayûm (Egypt), Somaliland and India; Neolithic antiquities in Malta; Fossils from the Auvergne; Chronology of the bronze civilisation in Italy; The early history of copper money; The latest prehistoric finds in Denmark; Survey of results of prehistoric archaeology in Russia; The original population of the Mediterranean seaboard; The peoples of prehistoric Portugal; Sources of Etruscan civilisation; Balkan influences on the culture of the first epoch in the iron age; A new classification of the Minoan periods in Crete; Relations between ancient Egyptian civilisation and that of the Eastern Mediterranean; Burials at Mycenae in relation to Cretan culture; Hellenistic pottery in Sicily; The development of brick-faced concrete construction; The head of Apollo from the Mausoleum; Art in Gaul after its conquest; Recent excavations at Paestum, Cumae and Pompeii; Relations between Greece and Carthage; Influence of Greek law upon the Roman law of inheritance; The Roman organisation of the corn-supply from Africa; Linguistic usages in official correspondence in Egypt under the Roman empire; Inscriptions from Algeria; Latin inscriptions from Morocco; Peculiarities of the double flute in antiquity;

The extent to which Roman civilisation transformed local civilisations in different provinces of the Empire; Temples upon Roman coins; Cowries and their substitutes as a currency in ancient China; Medieval money in Southern Italy; Astral symbols on ancient Babylonian boundary-stones; Religion at Palmyra and the cult of Saturn in Roman Africa; The Deity with the conical cap; Belief in demons in primitive Indo-Germanic times; Russian and Roman household-gods from an archaelogical point of view; Polynesian folklore; The Roman boundary in Tunis, Algeria and Morocco; Roman Savoy in the light of recent excavations; The aqueducts of old Rome; The buildings of Augustus on the Palatine; Fresh discoveries in the catacombs; The symbol of the fish in early Christian art; The technique of the portraits of Byzantine emperors; On forged antiques; Italo-Greek influences upon Celtic civilisation; The part played by the Roman army in diffusing certain worships; Art in the first century of Islam; On the organisation of archaeological teaching, research and publications.

This representative list, long as it looks, will not, I hope, be found too repellent, and is, in any case, short when compared with the full programme of the Congress. A brief glance through it will serve to indicate the geographical, historical, ethnological and artistic diversity of interests to which archaeology must make appeal. It would be invidious to characterise individual papers; but, without instituting odious comparisons, one may safely allude to some of Commendatore Boni's lectures, accompanied by lantern-slides (*proiezioni*) and by actual visits to the scene of his excavations in progress upon the Palatine hill, as giving some of the freshest results in ancient topography, all the more vivid and interesting that the work was proceeding before one's eyes.

Something of the same vivid interest attaches to the two ancient sites visited by the Congress as a whole; for both at Cerveteri in Tuscany, and at Ostia near the old mouth of the Tiber, labourers were actively engaged in excavating.

Cerveteri is the descendant of the ancient Caere, famous from early times for its close political connection with Rome. The members of the Congress reached it by taking train to Palo, which is 30 miles by rail from Rome, and then proceeding with much patience and high expectations, in leisurely bullock-carts to the picturesque little walled town with its single entrance, there to be received by a band and the inhabitants holding *festa*, and then to drink a *vermouth d'honore* presented by the local *sindaco*. The archaeological interest of Cerveteri lies in the necropolis, an elaborate system of tombs, near the modern town, hewn out of rock or contained in conical earth mounds Some of these grottos have been long opened and described; but some tombs are quite newly uncovered under government direction. Though less elaborately decorated than many Egyptian tombs such as (to take very fresh examples) those investigated at Meir, still these sculptured burial-places possess great value for their bearing on Etruscan art and civilisation of the seventh and sixth centuries B.C. The different grottos, lit by electric light, as one finds on descending into them, have been named after some distinctive decoration in each--the Grotto of the Shields, of the Banquet, of the Inscriptions, or of the Bas-reliefs. The last-mentioned (*grotta dei bassorilievi*). excavated about two generations ago, is particularly interesting for its two lionesses, at the head of a flight of steps, designed to guard the tomb in a manner that recalls the lionesses at Mycenae and elsewhere. Separately situated, but also included in our programme, was the Regolini-Galassi sepulchre, where the roof has been vaulted by the gradual approach of lateral walls—a system comparable to that adopted in the famous gallery at Tiryns in Greece.

For an inspection of the ancient harbour-town of Ostia it is most usual to enter from the N.E. corner through its old Porta Romana. The Congress arrangement was to enter at precisely the opposite end, namely the S.W. corner, close to the line of the littoral in classical times, though the remains of this vanished sea-port are now a considerable distance from the sea. The *Congressisti* approached and inspected Ostia in three groups Italian-speaking, French-speaking, and German-speaking Rail from Rome to Fiumicino, automobiles across the *Insula Sacra*, and a great ferry-barge over the Tiber were the means whereby one reached what was once almost the sea-front of a busy commercial town.

Almost directly after leaving the ferry, the visitors found themselves among the old shops and store-chambers furnished with huge jars for oil, and thence proceeded to more imposing edifices such as that named, on doubtful grounds, an ' imperial palace", the temple of Vulcan; the forum; the theatre; the baths; and the barrack of the firemen (*vigiles*), and so by now deserted streets to the ancient egress from the town towards Rome, where the street of tombs began outside the walls. A visit to Ostia must, of course, be supplemented by study of many of its artistic and domestic remains now housed in museums at Rome; but there is a great deal to fascinate one still left *in situ*, *e g.*, many quite legible inscriptions; or the large and well-preserved mosaic of black and white *tesserae* representing a sacrificial scene, inside the barrack of the firemen; or the statue of Victoria; or the instructive water-pipes; or the quite charming shrine of Mithras, where the mosaics on the stone benches represent the divinities of the seven planets; or, again, the figures in the baths worked in black *tesserae* to symbolize different geographical territories like Egypt and Spain. The ruins of Ostia and the finds among its ruins are highly significant documents for the private and social, international and economic, political and religious life of antiquity, especially in the first, second and third centuries of our era; and they possess a significance which even Pompeii cannot entirely eclipse; for Ostia was in close touch with Rome, and was at once metropolitan and cosmopolitan, while Pompeii might by comparison be termed provincial.

Note.- Dr. Wight Duff's paper concluded with the exhibition of over 30 slides prepared for the occasion. They were in four sets designed to illustrate:—

(1) Discoveries of recent years in the Roman Forum, including the Fons Juturnae and the Lapis Niger with the important monuments under it;
(2) The sculptured tombs of Cerveteri;
(3) The ruins of Ostia;
(4) The palaeography of certain pages from some of the most valuable Latin manuscripts in the Vatican, *e.g.*, the "Bembine Terence" (in rustic capitals of perhaps the 4th century A.D.); the palimpsest of Cicero's *De Republica* (in uncials of the 4th century A.D., with St. Augustine's commentary on the psalms written over and across them in demi-uncials of the 8th century); the "Augustean Virgil" (in square capitals of the 2nd or the 3rd century, A.D.) the "Vatican Virgil" (in rustic capitals of the 4th century, A.D.); the "Palatine Virgil" (in rustic capitals of the 5th century, A.D.); and the "Roman Virgil" (in rustic capitals of the 5th or the 6th century, A.D.).

SOME OBSERVATIONS ON A LEUCOCYTOZOON OF THE SISKIN (CARDUELIS SPINUS).

By THOMAS BENTHAM, B.Sc., Oxon.

[Read January 27, 1914.]

I have written this short paper to illustrate some few new points in connection with the morphology and life-history of the rounded form of Leucocytozoon occurring in the blood of Finches (Fringillidae).

In January, 1913, three Siskins (Carduelis Spinus) were obtained in Newcastle-upon-Tyne for the purpose of blood examination in the possible hope of finding trypanosomes. The search for these particular parasites was unsuccessful and cultures were tried in the case of one bird, but these also contained no trypanosomes. As the other two birds were dead when brought to me no further cultures were attempted. All three birds had been obtained from a bird-catcher near Brighton and were part of a batch of a dozen or so, all of which subsequently died within a few days of each other. Two of the birds received had been dead only a few minutes and were still quite warm. Both these last were adult males. The third specimen, a young male, was purchased alive.[1] From the first two birds were taken smears of blood from the heart and also from the lungs, spleen, liver and bone-marrow, and both birds were dealt with immediately they were received in the laboratory. All the internal organs appeared to be healthy, with the exception of the liver of one bird, which was infected with avian tuberculosis. Eimeria avium was found in small numbers in the alimentary canal. No external parasites could be discovered on any of the birds.[2]

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[1] On subsequent examination this bird was found to be free from parasites and was used for the purposes of a leucocyte blood count.

[2] True Finches seem to be free from ectoparasitic insects or arachnids. These are quite commonly found on Buntings (Fam. Emberizidae).

Slides were fixed with osmic vapour 3 per cent. solution for about 20 seconds. They were then again fixed in absolute alcohol for a quarter of an hour. The films were stained with Giemsa's solution (one drop to 1 cc. distilled water), for about sixteen hours. This length of time is absolutely essential during the winter months, although in summer a shorter duration will be found necessary. Leishman's stain was also employed but did not give such good results. Examination of the heart-blood shewed that leucocytozoa were present in large numbers. They could usually be seen in at least every other field under a one-twelfth inch oil-immersion, and in one case no less than six parasites were observed in a single field, four more lying in an adjacent field. Fig. 21 shews a group of three parasites lying close together in the same field of the microscope. The number of these parasites is seemingly very unusual, as Woodcock states that Leucocytozoon fringillinarum from the Chaffinch occurred in quite small numbers, twenty-five or so in one slide being quite an abundance, a more common number being five or six parasites in one film.[3]

A differential leucocyte blood count as taken from the young male mentioned above and the count for normal blood was as under: –

Small Mononuclears	12 8	per cent.
Large Mononuclears	5 4	,, ,,
Lymphocytes	61 5	,, ,,
Crystalloid Eosinophils	16 6	,, ,,
Coarse-grained Eosinophils	2 5	,, ,,
Mast-cells	1 2	,, ,,

No thrombocytes[4] were present and the small mononuclears were never spindle-shaped.

Fantham's percentage in the case of the Grouse (Lagopus scoticus) shews the large mononuclears to be much more abundant than in this case. He, however, did not include the small mononuclears in his blood count and probably included a good many of them in the count of large mononuclears. The two forms have been kept separate for reasons to be shewn later A count from the Goldfinch (Carduelis elegans) was almost exactly similar. In the infected birds the average Leucocyte count was as follows -

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[3] This is also the case in all Bramblings (Fringilla montifringilla) that I have examined, the Brambling being closely allied to the Chaffinch.

[4] I have only observed these in Sparrows and Weavers where they are of common occurrence.

Small Mononuclears	11·9	per	cent
Large Mononuclears	26·6	,,	,,
Lymphocytes	35·9	,,	,,
Crystalloid Eosinophils	5·5	,,	,,
Coarse-grained Eosinophils	1·1	,,	,,
Mast-cells	1·6	,,	,,
Infected cells	17·4	,,	,,

From this table it is seen that there is a great increase in the number of large mononuclears,[5] the small mononuclears being about normal in number If, as in the opinion of Woodcock and Fantham, all the infected cells are small mononuclears, we get an almost incredible increase of 17·4 per cent. in these, and if the small and large mononuclears be taken together there is an increase of nearly 40 per cent If a count be taken, excluding the infected cells, the percentage of small mononuclears is seen to be 14·5 per cent. which is not a great increase over and above the normal numbers.

As in the normal count no thrombocytes were seen. Numbers of red cells exhibited polychromatophilia but none of these, nor the normal erythrocytes, ever lacked nuclei. In infected birds nearly all the uninfected small mononuclears were markedly spindle-shaped, a phenomenon which, in my experience, practically never occurs in any Finch, but which is common in the blood of certain gamebirds, Gulls and Plovers.[6]

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[5] A similar condition found by Fantham in Grouse infected with Leucocytozoon lovati.

[6] Wenyon states that spindle-shaped cells occur normally in the blood of some birds.

DESCRIPTION OF THE PARASITE.

Almost all the parasites were small, rounded in form and measured on an average 4-5 μ in diameter. The largest parasites were invariably oval in shape and measured on an average 7 μ by 5 μ (Figs. 12, 13, 20). These large forms were nearly always macrogametocytes (see Fig. 7).

Female forms were slightly in excess of the males and in about the proportion—fifty-five per cent. females—forty-five per cent. males. These were always slightly larger than the males and their cytoplasm was more highly granular, contained less metachromatinic grains and stained a deeper blue by the Romanowsky method. The nucleus was small and compact with a more or less distinct karyosome which was either intra or extra-nuclear.

The cytoplasm of the male forms or microgametocytes stained but faintly, was non-granular, contained more metachromatinic grains and included a large, somewhat diffuse nucleus, with or without a karyosome. This nucleus was sometimes so large as to occupy almost the whole body of the parasite, there being but a small rim of cytoplasm present round the periphery of the parasite (see Fig. 6).

The parasites in the blood itself were either present free in the plasma (see Fig. 22) or were intracellular. Those free in the plasma were almays small rounded forms and were never found lying near the small mononuclear leucocytes. They were either completely free in the plasma lying away from the corpuscles or attached to and partially lying in the cytoplasm of all stages of red cell from erythoblast to basophile erythrocyte. Fig. 5 shews a young parasite lying partly in the cytoplasm of a basophile erythrocyte which was almost mature. In Figs. 11 and 15 parasites are seen lying adjacent to two forms of the corpuscle intermediate between the above and an erythroblast. Fig. 18 shews a macrogametocyte lying within an erythroblast, and in Fig. 22 is shewn a typical erythroblast such as is believed to be generally infected. When the full-grown parasite was intracellular it always occupied half the host-cell (Figs. 12

and 13), and the nucleus of the latter was invariably crescentic with its concavity turned towards the parasite. This nucleus was never reduced to a mere rim at the side of the parasite, as in Leucocytozoon fringillinarum. There is practically nothing to shew the nature of the host-cell, since, as soon as the parasite has become intracellular, the character of the nucleus of the former becomes entirely changed. It presents a homogeneous appearance, becomes much enlarged, and is devoid of all karyosomes or net-knots. In appearance it resembles the nuclei of erythrocytes which have either degenerated or have been altered by an artifact of preparation. Such erythrocytes occur in fair numbers in all slides of birds' blood and seem to possess but the faintest indication of an eosinophil cytoplasm. Woodcock definitely states that the host-cell of his Leucocytzoon fringillinarum is always a small mononuclear leucocyte. I am, however, more inclined to believe that Wenyon and Keysellitz & Mayer were partly correct in their statements that the host-cell is an erythroblast, because I have never seen any parasites lying in a small mononuclear leucocyte, but only in erythroblasts. When the parasite becomes larger, it is an impossibility to determine the nature of the host-cell. Furthermore, the extraordinary number of infected cells in the blood count and the almost normal percentage of the small mononuclears points to the fact that other types of cell are infected. Indeed a single parasite was found in a large mononuclear leucocyte in one of the liver smears.

I believe, as above stated, that the homogeneity of the nucleus of the host-cell is due to an artifact, dependent on the presence of the parasite, when the smear is made. The methods of smearing blood by glass slips, cigarette-papers, etc., though unavoidable, leave much to be desired. Liver smears, made by drawing a portion of liver across the slide with a pair of forceps, shewed more clearly the character of the host-cell nucleus, which then, in some cases, as Woodcock has stated, appears to be a small mononuclear, but not in all. An erythroblast has more small definite karyosomatic masses present than the nucleus of a small mononuclear leucocyte and in some cases these shewed up quite plainly in the liver smears (see Figs. 12, 13, 16, 19, 21), the presumed small infected mononuclears shewing a completely homogeneous nucleus. It is not beyond the bounds of possibility that, in large infections of this parasite, although mononuclears are chosen for preference, other cells, such as erythroblasts, are infected by force of circumstance.

Horn-like prolongations of the cytoplasm of an infected cell were never observed, although, as stated above, uninfected cells appeared to have these.

The parasite seems to be a true rounded form of Leucocytozoon such as that described as existing in the Chaffinch and Greenfinch, possibly a distinct species, not only because of its small size, but also on account of its action on the nucleus which it never compresses into a mere ridge at the side of the parasite. Furthermore, the fact that the Chaffinch Siskin, and Greenfinch belong to different genera should be taken into account in support of the above statement.

Contrary to Woodcock's statement many young forms of the parasite were found to be completely intra-nuclear in position (see Figs. 14 and 16) and nearly always situated exactly in the centre of the host-cell nucleus. Stages were found in which the host-cell nucleus was almost closed round the parasite leaving a narrow passage from the latter to the cytoplasm (Fig. 1).

This condition almost points to the fact that, in some cases in its earlier stages, the parasite is intra-nuclear and that as it grows it bursts from the nucleus and pushes it to one side. This, however, is not always the case, as can be seen from the contour of the nucleus in most of the diagrams. Occasionally, the parasite as it grows exerts a karyolytic action on the nucleus of the host-cell. This nucleus is seen to be broken up into several irregular rounded masses lying in their own cytoplasm, the Leucocytozoon always lying attached to the largest of these masses. This singular condition is peculiarly reminiscent of the action of Karyolysus

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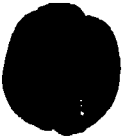

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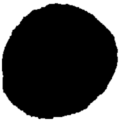

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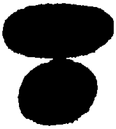

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although in this genus the nucleus of the host-cell is never so completely fragmented (Figs. 3 and 3).

Infection of a single host-cell by two parasites was very common. In this case the parasites were either close together and on one side of the nucleus (Fig. 14), or they occupied positions on opposite sides of the nucleus (Fig. 19). In these cases of double infection, the two parasites were almost always of the same size; not necessarily, however, of the same sex. Woodcock never observed any cases of double infection by the rounded form of Leucocytozoon, but this was in all probability due to the small number of parasites he was able to find in his smears. Where large infections of Halteridium are found in birds, it is fairly common to find corpuscles doubly infected and consequently there is absolutely no reason why such a state of affairs should not occur in Leucocytozoon. Thus it will be seen that the two parasites in all probability penetrate the host-cell on opposite sides, an impossibility in a small mononuclear leucocyte if we are to believe the statements "that the parasite always penetrates into the leucocyte on the side where there is most cytoplasm."

These parasites were exceedingly numerous in the liver smears and occurred in small numbers in those of the bone-marrow, spleen and lungs. In the liver the organisms were found mostly free from the corpuscles and were quite rounded in contour and similar to the free forms in the blood. They were, however, commonly found associated in pairs, whether free in the substance of the liver or attached to a host-cell. It was further noticed that large numbers of single parasites appeared to possess two nuclei sometimes quite separate (Figs. 9 and 10), or connected by a definite spindle (Fig. 8). These double parasites seemed therefore to be undergoing a process of binary fission. No schizogony was ever seen in the spleen or bone-marrow.

CONCLUSIONS

(1) The organism described is a parasite of the Siskin (Carduelis Spinus).

(2) The parasite, under the above conditions, attacks both immature red cells and small mononuclear leucocytes.

(3) It differs from Leucocytozoon fringillinarum (Woodcock) in its disposition towards the nucleus of its host-cell, and in its size, which is smaller than that of the fringillinarum.

(4) It may be intra-nuclear for quite a considerable period of its existence.

(5) Two parasites as in Halteridium are often found infecting the same host-cell.

(6) The parasite sometimes karyolyses the nucleus of the host-cell.

(7) Binary fission occurs in the liver of the host.

(8) As in the case of Leucocytozoon lovati of the Grouse (Lagopus scoticus) large mononuclear leucocytosis is markedly present in the blood.

(9) The parasite may be present in its host in enormous numbers.

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SOME FEATURES OF THE GLACIAL DEPOSITS AT THE TYNE ENTRANCE

By S. RENNIE HASELHURST, M.Sc., F.C.S.

[Read December 9, 1913.]

It has been pointed out in a previous paper[1] that the sites of early human habitations were often determined by a readily available water supply and a dry site. Tynemouth village and Percy Square, North Shields, are two instances. In this district these conditions postulate sand, and led me to suspect it prior to its exposure. Recent excavations have revealed sandy deposits of a later age than the "scarp" boulder clay. The locus of these excavations is the Eastern extension of the North Shields Fish Quay, where the huge boulder clay cliff has been scarped back to a gradient of 1 in 1. These sections have afforded unique opportunity of studying sequence in these deposits in an area where it is most difficult to obtain, due to the absence of clay pits and quarries and the "built up" nature of the vicinity.

The method of mapping.—As exposed, each section was carefully measured up in correct sequence, and these dimensions were then transferred to a true scale section drawn upon a roll of wall paper. This was then photographed, giving the result shown in Fig. 1. The obvious

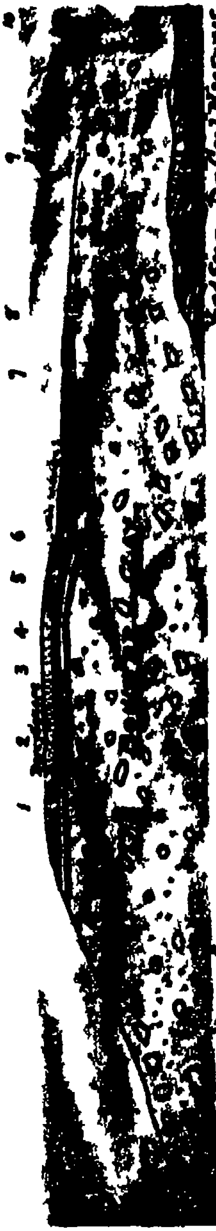

FIG. 1

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[1] Haselhurst, S. R. The Causes of the Tynemouth Landslips, 1913. Tynemouth Corporation.

advantage is that these deposits are shown in their exact topographical relationship and if we are to attempt a correlation of analogous deposits so clearly devoid of useful fossils the statistical as well as lithological correlation must be followed

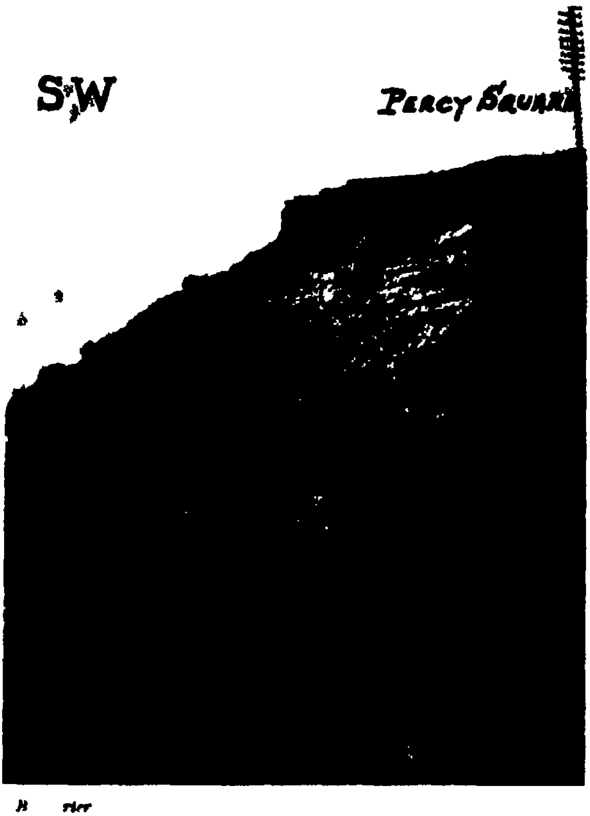

Fig 2

Description of the section —The section extends upwards of ½ mile from the Flats to Collingwood's monument (See map pp 150 1) It rises in parts to 110 O D consisting of sandy clays with partings of blue clay silt brown clay thick deposits of sand (10 15) underlain by brown plastic clays corresponding to those cited by Merrick[2], reposing upon the "scarp" boulder clay (Fig. 2.) Glaciated stones and pebbles are singularly absent, although some waterworn fragments of striated shale have been found. The deposits of sand are false bedded, containing fine partings of comminuted coal and dendritic, stalactictic tubes of calcium carbonate. The brown, plastic clays underlying these sands are finely stratified, and contain numerous fragments of drift-wood, identified by Whitehead as birch. (Fig. 3.)

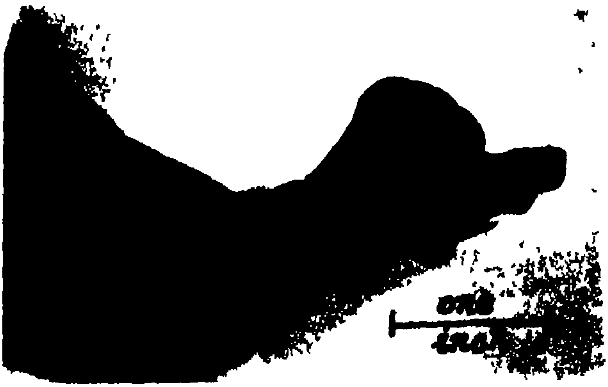

FIG. 3.

They are chiefly remarkable for a unique series of perpendicular, cylindrical concretions resembling huge bolts 18″ and upwards in length, 1½″ in diameter, with a persistent woody core about ⅛″ in diameter. The concretionary action has not been selective, fine stratulæ of clay and sand composing them alike. These concretions are concentrically coated or tunicated. There is an abundant supply of water in the overlying sands.

Interpretation of the beds: Origin of the concretions.—Their whole appearance points out that they were ***quietly deposited*** either *in a low-grade stream at a high level* or *in a lacrustine depression or scoûr in the scarp clay*. It is immaterial whether they were ponded back by a tongue of ice in the Tyne valley after the ice-barrier fashion of the Marjelensee, in which case we could not expect similarity of sequence on the south bank, although it might easily obtain in adjacent valleys on the north bank. As their locus is near a large town, I cannot at present record "*strand linien*," nor is there any basal deposit such as might reasonably result from melting ice. The birch fragments are driftwood lying 15′ below surface, and the rootlets forming the concretions are certainly *in situ*. Due to the lack of transverse sections, I cannot at present define the topographical limits.

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[2] Merrick, E., *Proc. D. U. Phil. Soc.*, vol. iii., pt. 3, p. 142.

Regarding the concretions, I offer the following explanation:

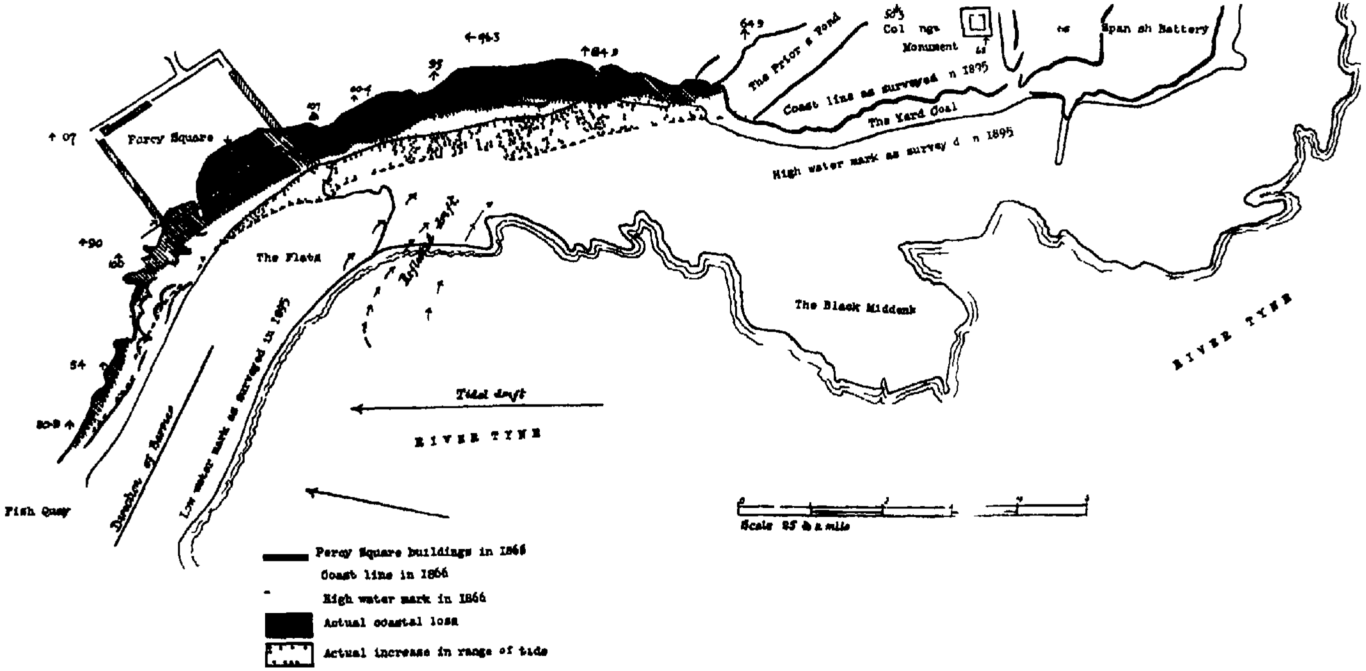
The growing rootlets penetrated the beds irrespective of their composition. The root pressure, increasing with age and decreasing with decay, produced a permanent state of strain in the clay and sand. This pressure decreased to a minimum from the central axis as a radial force, thus forming at zero the boundary of the potential concretion. On decay and death, the pressure of the rootlet would be removed and *either*,

(*a*) The mineralizing waters descended these tubes from the sands above, and were carried into the concretion along the radial lines, thus giving rise to tunicated zones, *or*,

(*b*) They were driven along the sandy stratulæ and segregated inwards towards a centre by a joint action of hydrostatic pressure and capillarity of the rootlet

The dead rootlets certainly offered a final channel for the solution.

MAP SHOWING SITE OF GLACIAL LAKE AT TYNEMOUTH & COMPARATIVE EROSION



On the Rate of Recession of the North Bank of the Tyne at Tynemouth.

By S. Rennie Haselhurst, M.Sc., F.G.S.

[Read December 9, 1913.]

Some further work on coast erosion is given in the map. (Fig. 4.) To extend the North Shields Fish Quay, a large area known as The Flats, west of the Black Middens, has been reclaimed. A massive sea-wall bounds this, and is opposed to the direction of the tidal drift, deflecting it N.E. The river is thus confined to smaller limits, and there is a concomitant increase in the range of the tide in the vicinity of the Fish Quay. A direct result is that the cliffs described in the preceding paper are being destroyed at an increasing rate by tidal deflection, increased range and "sub-aerial" land sliding. It seems a paradox, but the same community recently obtained an injunction *to restrain* an attempt at increasing the range of the river, and have achieved the same object by municipal improvement. Coast erosion here is due to a combination of phenomena exactly similar to that cited by my co-worker, Walmsley[1], at Robin Hood's Bay, and is of exceptional interest, as it is primarily due to marine and not sub-aerial agency, exemplified in the Tynemouth landslides. The map bears interesting comparison with Walmsley's (*op. cit.*), and shows for the first time as regards Northumberland a definitely measured rate of recession corresponding to 35 yards average in 29 years, a superficial loss of 4·67 acres, which taken in proportion to the area shown in the map, is very great.

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[1] Walmsley, L., *Naturalist*, 1913, Aug. 1st, p. 280.

THE APPLICATION OF GRAPHIC METHODS TO A CASE OF DAMPED OSCILLATORY MOTION.

By F. H. ALEXANDER, M.Sc.

[Read December 5, 1913.]

The paper of which an abstract is here given, dealt with the method of graphic analysis described in detail by the late William Froude, F.R.S. in a paper read before the Institution of Naval Architects in 1875.

In a curve of motion in one plane the value of $\frac{ds}{dt}$ at any instant can be represented to scale by the perpendicular of a right-angled triangle, the base of which represents a finite time interval; and if, at instants separated by equal finite time intervals successive values of $\frac{ds}{dt}$ be represented upon the same perpendicular, then the difference in the length of any two successive values represents the mean value of $\frac{d^2s}{dt^2}$ during the time interval. In other words the slope of each hypotenuse represents a velocity, and each change of slope represents an acceleration during the time interval represented by the base. Such a figure is called a polar diagram.

If the second integral of a given curve be obtained, it can be shown that the intersection of any two tangents of the second integral curve lies upon the same ordinate as the centre of area of the given curve between the limits at which the tangents are drawn.

By the aid of the above propositions it is possible to determine graphically the motion of a body in one plane when acted upon by known forces in cases where the analytical treatment of the problem is prohibitively difficult or is impossible owing to interdependence of terms.

Such a case is illustrated by the rolling of a ship in a series of waves Here the resultant couple acting to produce angular motion at a given instant is the algebraic sum of three couples viz (1) That which tends to place her with the mast vertical (2) That which tends to place her with the mast normal to a certain subsurface of the wave (3) That which is due to resistances of water and air The first two couples depend upon angular displacement from a position of equilibrium and of these the second only can be expressed in terms of time

The resistance couple varies partly as angular velocity and partly as the square of that velocity and its evaluation depends upon constants derived by experiment upon the ship when set rolling by artificial means in still water

It is possible to represent separately (1) the angular acceleration due to the couple which tends to restore the ship to a position of equilibrium the curve being obviously what is known as the ship s Curve of Stability but modified in scale (2) the angles which the normal to the wave surface makes with the vertical in terms of time (3) the negative accelerations due to resistance in terms of angular velocity

The operations involved in tracing the motion bring together the three couples just mentioned as follows — A polar diagram is used the base of which is parallel to that of the motion diagram hence at any instant the tangent of the curve of motion is parallel to the corresponding hypotenuse of the polar diagram At a given instant the motion diagram shows the angle θ which the ship is making with the vertical and also the angle a which the normal to the wave surface is making with the vertical Thus the angle $\theta + a$" is the angular displacement from a position of equilibrium and the angular acceleration towards that position may be obtained from the separate modified curve of stability Again where the hypotenuse of the polar diagram cuts the perpendicular and therefore indicates the instantaneous angular velocity, it is possible to measure to scale the negative angular acceleration due to resistances. Combining these values the resultant acceleration at the instant is determined, and it is therefore possible to accompany the curve of motion with a curve of accelerations. The curve of accelerations is carried forward tentatively so as to indicate an approximate mean acceleration during a small finite time interval, and by transferring this acceleration to the perpendicular of the polar diagram, a new velocity is obtained at the end of the time interval. Then a tangent is drawn in the motion diagram parallel to the new hypotenuse in the polar diagram, and a corrected value is then obtainable for the acceleration curve at the end of the time interval. If this corrected value differs from that tentatively used, it may be needful to perform the operation again for the time interval concerned. Care must be taken to make the successive tangents of the curve of motion intersect in accordance with the second proposition referred to earlier.

In practice it is convenient to make the finite time interval for each operation equal to $\frac{T}{10}$ and to make the base of the polar diagram represent $\frac{10}{\pi}T$, where T is the period of a single unresisted swing through a small angle in still water.

It is not possible within the compass of an abstract to explain the process in detail, but those who desire may find a complete description in the paper by Mr. Froude already mentioned.

Resisted oscillations have such an important place in modern physical science that the method, here alluded to, is worthy of attention because it is applicable to the most complicated cases.

A CONGLOMERATE UNDER THE BOULDER CLAY OF THE SUNDERLAND DISTRICT.

By S Robson, M Sc

[Read December 9, 1913.]

Under the boulder-clay at several places in the Permian near Sunderland there occurs a conglomerate of an unusual derivation. It is found intermittently distributed over the rocks exposed in section along the coast from Souter Point to Ryhope and can also be seen in some of the "denes."

As its presence has apparently not received attention it has been thought worth while to describe it briefly.

The conglomerate is made up of the diverse materials to be found in the boulder-clay—fossiliferous limestone, whin, quartz pebbles, etc., and is obviously derived from this superficial deposit. In every case, in which it has been observed, it lies on a horizontal surface of rock, generally soft or much brecciated, into which it has been forced by the weight of the overlying clay and as a general rule is only a few inches thick A recent cliff fall at Ryhope shewed large masses of broken rock held to the clay by means of the cementing material of the conglomerate which had bound the lower boulders of the clay to the rock surface. In places also large isolated blocks of whin are cemented in position and may be seen on the sea-shore firmly fixed to the underlying rock long after the covering of boulder-clay has been washed away.

The following analyses of the cementing material and underlying rock at one patch shew that the actual binding agent is calcium carbonate.

No. 1 is carefully selected cementing material from the conglomerate at Souter Point. Nos. 2 and 3 are samples of the rock underlying this conglomerate.

	No 1 (Cement)	No 2 (Marl)	No 3 (Soft Rock)
H_2O ...	3·96	1·45	47·24
CO_2	42·40	45·99	
SiO_2 (insol.)	2·41	0·31	0·46
$Al_2O_3 + Fe_2O_3$	2·34	1·50	1·41
CaO . . .	33·71	30·73	31·25
MgO .	14·38	19·86	19·20

The silicon in the cement was present almost entirely as sand grains and does not appear to be the cementing agent in this particular example though the patches vary so much in character that it cannot be said that the cementing material is always calcium carbonate.

This excess of calcium carbonate is probably derived from the limestone boulders in the boulder-clay. Water found its way down cracks in the clay and after passing over the innumerable boulders and fragments of limestone of this deposit reached the lower boulders and small stones which had been forced into the rock surface. These were thus cemented to the rock and patches of conglomerate of varying size formed

NEO-REALISTIC THEORIES OF MIND OR CONSCIOUSNESS.[1]

By R. F. A. HOERNLÉ, M.A., B.Sc.

[Read March 10, 1914.]

INTRODUCTION.

Since the beginning of this century the philosophical world has been full of movements of unusual interest and promise. First, the academic calm was stirred by the *Pragmatism* of William James with its off-shoot, the *Humanism* of Mr. F. C. S. Schiller. Then came the discovery of Bergson, who, after having for many years thought and written in comparative obscurity, suddenly experienced a meteoric rise to international fame. Whatever value we may put upon his theory as a whole, some of his profoundest ideas will, I am convinced, have an abiding influence on philosophical speculation long after he has ceased to be the fountain of wisdom for soulful ladies of fashion. Like all fresh and original minds, Bergson is hard to fit into the pigeon-holes of our traditional classification of philosophical systems, but, if he is to be fitted in, he must be called an

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[1] In order to reduce the references in the body of the following paper to the indispensable minimum, it will be best to give, at the outset, a list of the chief books and articles which I have had in mind in writing. They are:

(1) For the Oxford School of Neo-Realism: Mr. H. A. Prichard's *Kant's Theory of Knowledge*.
(2) For the Manchester School: Professor Alexander's articles during recent years in the *Proceedings of the Aristotelian Society*, in *Mind*, and in the *British Journal of Psychology*.
(3) For the Cambridge School: Mr. Bertrand Russell's *Philosophical Essays* and *Problems of Philosophy*, and articles by him and by Mr. G. E. Moore in *Mind*, and in the *Proceedings of the Aristotelian Society*.
(4) For the American School: Professor R. B. Perry's *Present Philosophical Tendencies*, and the volume entitled *The New Realism*, containing essays by six American Realists.

Idealist. Yet, at the same time, the other and older forms of *Idealism* have by no means lost their vitality or their fruitfulness. It is enough, within the English-speaking world alone, to point to the recent Gifford Lectures of Professors James Ward and Bernard Bosanquet, and, in America, to the writings of Professors Josiah Royce and Hugo Munsterberg. And, lastly, within quite recent years we have been able to welcome in *Neo-Realism* a vigorous movement, which has already split up into several different schools, and which, aggressive in its challenge and ambitious in its constructive programme, bids fair to play a far more important part on the philosophical stage than either Pragmatism or even Bergsonianism [2] Its challenge is directed against Idealism in all its forms. By its very name it claims to champion *reality*, to re-assert the existence of an independent object-world against what it takes to be the theoretical denial of that world on the part of Idealism. Now, in the clash of philosophical arguments, as in other battles, the dust is apt to fly, and a condition results such as that which made Berkeley complain that "philosophers first raise a dust, and then complain that they cannot see." The points of issue become confused, and the arguments on both sides correspondingly irrelevant. This is what I believe to have happened to some extent in the recent controversies between Idealists and Neo-Realists. Hence there is, I conceive, a real and pressing need for a precise definition of, and orientation about, the points of issue. The time has come for a laying of the dust, and it is as a contribution to this humble, but necessary, work that I propose to offer the following discussion of *one* only, but that the most fundamental, of the problems in dispute, viz., the problem of Mind or Consciousness in its bearing on the theory of the nature of reality. The issue may be, briefly and provisionally, stated thus: Idealists of all shades agree in construing the universe as, in last analysis, mental; as a mind or a system of minds. Realists of all shades agree in holding that reality is different from, and independent of, mind; or, more accurately, that mind, which is, in some sense, undeniably real itself, is only a part of the total reality, and, moreover, a part which is not necessary to the existence and nature of the rest. This, then, is the problem which forms the subject of this paper.

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[2] If this estimate of the importance of Neo-Realism be challenged as an exaggeration, I should reply (1) that Realism is the natural reaction against the apparent paradoxes of Idealism, and that attempts to maintain it, in some form or other, will therefore continue to be made as long as Idealism holds the field; (2) that modern Neo-Realism, in particular, is important as introducing into philosophical speculation many of the recent results and theories of Natural Science, Mathematics, and the Logic of Relations, (3) That a movement led by such thinkers as Mr. Bertrand Russell and Professor Alexander in England, Professor O. Külpe (the first volume of whose *Realisierung* appeared last year) in Germany, and a vigorous band of young philosophers in America, is bound to have a considerable influence.

PART I.—GENERAL SURVEY.

Nothing, at first sight, may well seem more in keeping with commonsense than the position of the Realist, nothing a wilder paradox than that of the Idealist. The distinction between minds and bodies, as the two chief constituents of what we ordinarily call the "real world," is one which the very words of language teach us to make, and which constant use has made so familiar that, ordinarily, we have even ceased to reflect upon it. Most of us, if not versed in philosophy, may even discover a sneaking tendency in ourselves to look upon the body, because it is solid and substantial and space-filling, because, in short; it is material, a thing to be seen and felt and handled, as somehow *more real*, more securely *there* (as it were), than a thing invisible, intangible, immaterial, "abstract," like the mind or "soul." Religious motives may lead some to reverse this valuation, to put the body low in the scale of reality because it is perishable, and the soul high on the ground that it is immortal. But when we approach the question from the scientific point of view, we tend to be impressed rather by what I will call the *continuity* of the body with the material universe and the *discontinuity* of the mind, both within itself and in its contact with other minds. The body, though subject to dissolution as an aggregate of elements, none the less, just through these very materials of which it is composed, shares in the stability and permanence of the physical universe as a whole. This stability, of which the law of the conservation of matter is the most general formula, has found expression in the scientific concept of the material world as a closed, self-contained, mechanical, material system; a system in which minds have no share or place, which was before ever a mind had evolved, which will be long after all minds are extinguished, and which behaves according to its own laws, independently of the presence or absence of minds. We need but go one step further, and either deny the existence of minds altogether, or treat them as mere idle, ineffective by-products of the bodily machine, to have the theory of reality which is called *Materialism*. In the continuous material system which this theory assumes, minds seem but like discontinuous sparks, kept flashing, here and there, by bodies of a certain structure,—sparks which glimmer for a brief space and then are gone for ever. Not only do they seem to have no continuity with each other: even within their own short span of existence they are subject to constant interruptions by sleep and unconsciousness. What tides us over these intervals, these breaks in our mental life, except the continuity of the material frame, the body? Reflections such as these almost inevitably lead one to ascribe a higher reality to the body, and in general to the material universe, than to the mind—to call the material universe "reality" par excellence, and to treat it as complete in itself, and independent of mind.

This was the position of the *Old Realism*, which was thus, in effect, more or less deliberately, more or less outspokenly, *materialistic*. We shall recognise it wherever, in any philosophical theory, we find these two orders of existents asserted, a material order and a mental order, with the implication that the former is fundamentally more real than the latter, and therefore the latter dependent on the former, but not *vice versa*. It is a view which we can easily identify by asking the *fundamental test-question* which I propose to address to all Realisms: supposing there were no minds at all, supposing them all annihilated, how much would be lost out of our universe, how much would remain? Whatever we take as remaining after the subtraction of minds—*that* will be our "independent reality," and the exact character of our Realism will vary according to the way in which we conceive this independent reality.

Neo-Realism differs from the older forms, not in the fundamental principle of asserting a non-mental reality, but in refusing to regard this reality as exclusively material. Material objects are to it only one kind, and perhaps not even an ultimate kind, of reality. There are *non-material objects*, which, for all their non-materiality, are yet non-mental, *i.e.*, are in character and existence independent of mind in the sense required by Realism. True, the different forms of Neo-Realism in England and America differ widely from one another in the extent to which they go beyond Materialism. The *Oxford* Realists, led by Mr. Prichard, are nearest to the Materialists: in fact, their independent reality is just the material system of the Physicists, a universe of molecules, atoms, electrons, etc., a universe without sound or colour or taste or smell, in short, a universe stripped of its *secondary* and reduced to its *primary* qualities. Again, the *Manchester* school, represented singly but mightily by Professor Alexander, is materialistic in temper, for it speaks of real objects as "physical" and as "bodies." Its Materialism, however, is, like Sam Weller's knowledge of London, "extensive and peculiar," for it treats the objects of dreams, hallucinations, imaginations as no less "physical" than the things of our waking perception. Only when it comes to *numbers*, does it confess itself puzzled. For they are clearly not physical, and yet are "real" and "independent." The last of the *English* schools, viz., the *Cambridge* school, led by Mr. Bertrand Russell and Mr. G. E. Moore, is very much less materialistic than the previous two,[3] largely because, through Mr. Russell, it has been profoundly influenced by modern mathematical and logical theories, and among the latter, more especially by the writings of Meinong.

Now, it is just here that, as it seems to me, we come quite clearly upon the decisive break between the Materialism of the older Realists and the fresh conception of reality of the modern Realists—the fresh conception which justifies the epithet "New." The decisive point, in principle, is the recognition of reals which are non-material, without thereby becoming "mental," which are, therefore, both non-material and non-mental. For want of a characteristic word we may call them, with the *American* school of Neo-Realists, "neutral" It is to be set down to the credit of the American Neo-Realists that they have recognised the character of the new principle more clearly than the English schools. Yet even they do not all seem to have appreciated its significance or scope. Many even among them tend to gravitate back towards too exclusive a pre-occupation with reals which are material. But the principle itself is clear enough. It is nothing but the recognition that every form of apprehension has its "object," and that the "existence" or "subsistence" of this object can be distinguished from the occurrence of the apprehension. The principle might perhaps be called the *principle of the independence of the cognitive object.* It posits as fundamental the "objectivity" (—the "Gegenstandlichkeit" of German logicians) of whatever we are aware of. To be aware "of" something implies that there *is* something of which we are aware. The exact status of this something in the realm of being may be matter for discussion, but its being there, as that which we apprehend, is guaranteed by the apprehension itself. Anything whatsoever, then, which we can in any way apprehend at all—any object in this wide and inclusive sense—is real (*i.e.*, exists or subsists), and is independent in the sense required by Realism. That is what the principle claims, or would claim, if all Realists fully appreciated its significance. Within the general "universe of being" which is thus posited, we can make what distinctions the character of the objects appears to demand. We may class them as material and non-material, as facts and fictions, as appearances and realities, as abstract and concrete, as truths and errors. But, in so far as they all are objects, they all alike exist or subsist independently of the mind, *i.e.*, of apprehension, thought, knowledge.

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[3] A good test of this is Mr Russell's attitude on the question of the reality of *universals* (Problems of Philosophy, ch ix), or again, his theory of Sense-data (*ibid.*, ch. i.-iii.).

This seems to me the really important principle in Neo-Realism--important, both because it frees Realism from the fetters of a Materialism which had become a mere effete prejudice, and even more because it affords, as we shall see, a basis for fruitful discussion with every kind of Idealism which calls itself "Objective" To be sure, Realists themselves are often still very far from appreciating this fully, partly (as I suggested just now) because they have not yet shaken off the old habit of assigning to the problem of the material world a prerogative place, partly because the application of this principle with ruthless consistency to all objects whatsoever runs counter to many other established habits of thought, *e.g.*, it involves "objective falsehoods." It is sufficient, however, merely to think of the whole range of mathematical entities to have an example of objects which may be said to subsist, and therefore to be real and independent, without being either "material" or "mental" in any sense ordinarily attached to these terms.

At this point we shall be well advised to take a fresh breath, for the next big wave of the argument is about to rise up and smite upon us.

Those with a keen ear for dialectical over-tones will have noticed, that in the course of the last argument the relation of real object and mind has transformed its character. Such terms as "object," "cognitive," "apprehension," "knowledge" were repeatedly used. But with these very terms the problem has been put in a fresh form. We had begun by considering the current distinction of "minds" and "real things" *without reference to* the special feature that "things" are said to be "known" by "minds." But from the moment that we began to treat "things" as "objects" which, through perception and thought, are "apprehended" or "known," the whole problem shifted under our hands. The question is no longer how to distinguish in our universe non-mental realities from minds, both being admitted to occur, to co-exist, and even to co-operate as, *e.g.*, in the "psycho-physical" human personality The question is now the much narrower, but also much profounder one of *the relation of realities, regarded as objects of knowledge, to the minds which are said to know them.* In short, we have broached what has often been declared to be the central problem of modern philosophy, viz., the *theory of knowledge* We are dealing with the cognitive situation or relation: minds know objects, objects are known by minds. This apparently commonplace and harmless situation gives rise, as is well known, to some of the most puzzling problems of philosophy. We can now re-define the issue between Realism and Idealism in a preciser form Idealism, according to the interpretation of its Realist critics, holds that to be real is to be known, to be object of a mind, that this relation to, or dependence on, a knowing mind is essential and constitutive alike for the existence (or subsistence) and for the nature of the object known. Idealism, on this view, extends Berkeley's *esse est percipi* to judgment and inference: it is the thorough-going and consistent elaboration of a monstrous initial assumption, viz., that things are "real" or "have being" only when and as long as some mind perceives or thinks them. That the language commonly employed by Idealists lends colour to this interpretation, is undeniable, cp., *e.g.*, the phrase "reality is our intellectual construction." But I shall try to show below that the interpretation is, none the less, misleading. However, for the present, the point to note is that Idealism is held to identify reality and mind in the sense of dissolving our stable, systematic universe, *e.g.*, the world of nature, into the dream-like tissue of the flux of perceptions and thoughts. There is nothing real, Idealists are understood to say, except what minds perceive and think when and so long as they perceive and think it. Truly this may seem a reduction of reality to mind which dissipates reality, and which, instead of real things, leaves nothing but mental states in our hands. No wonder that the Realist rises up in defence of reality and proclaims its independence of mind in the sense that it exists and has a definite nature of its own, whether it is known or not. Thus Professor Perry declares that Realism "means that things may be, and are, directly experienced without owing either their being or their nature to that circumstance."[4] Professor Montague states the same view more fulsomely: "Realism holds that things known may continue to exist unaltered when they are not known, or that things may pass in and out of the cognitive relation without prejudice to their reality, or that the existence of a thing is not correlated with, or dependent upon, the fact that anybody experiences it, perceives it, conceives it, or is in any way aware of it."[5] The English Realists speak in a similar strain. Thus Mr Prichard says: "Knowledge unconditionally presupposes that the reality known exists independently of the knowledge of it, and that we know it as it exists in this independence."[6] And again: "We can no more think that in apprehending reality we do not apprehend it as it is apart from our knowledge of it, than we can think that existence depends upon our knowledge of it."[7] Professor Alexander declares the 'vital question' to be whether objects 'are independent of mind,' and proposes to use the terms 'non-mental' or 'external' to express this independence.[8] These quotations may suffice to illustrate the insistence with which Neo-Realists of all shades uphold the conception of a universe which shall be "real" in the sense of "independent of mind," where "mind" means knowledge or the processes (or activities) of knowing (perceiving, thinking, etc.) and where "independent" means that those processes make no difference[9] to the nature or existence of the real.

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[4] *Present Philosophical Tendencies*, ch. xiii, §6, p. 315.

[5] *The New Realism*, appendix, p. 474.

[6] *Kant's Theory of Knowledge*, ch. iv, p. 118. [7] *Ibid.* p. 119.

[8] "On Sensations and Images," *Proc. Arist. Soc.* 1909/10, pp. 5, 6.

Now, in order to deal with the issue in this form, we must once more take a survey for orientation. And our best clue will be to put the question: *What do we mean by a mind?* What does it do? What is it made of? What are its constituent elements? To guide us to the answer, let us again apply the test-question suggested above: What would disappear from our universe if minds were wholly to disappear out of it? That is, in effect, the subtraction which the Oxford Idealist, Mr Prichard, asks us to make when he lays down the principle that we must treat as mental everything in the universe which would disappear with the disappearance of minds. Very well, let us make the subtraction.

(*a*) Minds are commonly said to feel, to think or know, and to will, or to consist of feelings, perceptions and thoughts, and volitions. Rough and ready as this threefold division is, it has well-established philosophical tradition in its favour, and it is, moreover, endorsed by most modern psychologists. It will serve sufficiently well for our present purpose, and so we shall conclude, in the first instance, that the disappearance of minds from the world would mean the disappearance of every kind of feeling, thought, and will—in short, of "consciousness"- but would leave everything else as it is.

(*b*) But *is* there anything else? That is just the point at issue. Let us try further. Leaving aside feelings and volitions, on the ground that the dispute is concerned only with "knowledge" of real objects, we shall say that to "know" is to perceive and think It might be objected that this is too unqualified a statement. To know, it may be said, is to perceive and think *truly*, to perceive and think things *as they are*. Not all perceiving and thinking satisfies this test. We mis-perceive, we mis-think. Mistakes and errors are not "knowledge." But this distinction, again, we may neglect, for even when we think falsely, we do undeniably think *something*: our minds are not an empty blank. There is, cognitively speaking, an *object*—misconceived and distorted if you will, but still an object. For this reason, we shall take knowing in this wide sense of "apprehending," by perception or thought or imagination or in any other way, any "object" whatsoever, and we shall ask again: what disappears when knowledge or knowing is subtracted from the universe?

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[9] Professor Perry protests against the phrase "making no difference" (*New Realism*, p 104), but it seems a fair paraphrase of the view expressed in the quotations given, and it has the authority of Professor Royce (*The World and the Individual*, First Series, pp. 118-123), and Mr. Joachim (*The Nature of Truth*, pp. 33, 58).

And here we are at the parting of two ways "Knowledge," I said, or "knowing." That suggests the distinction between "knowing" as the mind's *activity*, and "knowledge" as *what is known* (=the object). The conclusion, paradoxical in language, would be that, though knowing disappeared, knowledge would remain. And, further, it would follow that what is known and what is real are, in principle, one and the same, and Idealism is on the point of being justified. But the solution, alas, is not so simple. We are forgetting that this simple distinction between the activity of apprehending and the object apprehended, though defended by recent theory, runs counter equally to everyday thought and to much authoritative philosophical speculation. For example, when we dream or indulge in the play of imagination, we do not merely regard the activities of dreaming or imagining as mental, but the dream-objects and imaginations as well. In a word, we recognise *objects* which are "mental" and would disappear with the disappearance of minds. And these "mental" objects we class as "unreal" by distinction from those which, though equally capable of appearing within the field of apprehension, are "real." And of these "real" objects we tend to say that in becoming objects they do not become 'mental" in short that they are non-mental or at least not merely mental, not such as to depend for their existence and character on the activity of the mind[10] Our conclusion would

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[*] In this passage the terms mental etc have been used in their current commonsense meaning But to a critical reader it is obvious that this sense is full of ambiguity and where there is ambiguity there the situation is full of dialectical possibilities For example the current distinction of minds and bodies and the identification of bodies with material non mental real objects is a piece of naive metaphysical dogma which takes no account of the paradoxes of the theory of knowledge It does not reflect that bodies and minds in order to be distinguished must both be thought of i e must both become objects must enter into cognitive relation to a mind But this means that the mind is capable of apprehending both itself and what is not itself (its other) and of distinguishing itself from its other —Or again we distinguish with illusory ease in popular theory between 'things and thoughts and since the mind thinks or has thoughts we tend to identify the distinction between things and thoughts with the distinction between objects (non mental) and minds But thoughts are of or 'about things What then exactly is the distinction between a thing and the thought of that thing? Or between the thing in itself and the thing as thought?—And yet again as we found just now a mind can make an object of itself i e form a thought of itself But if so the distinction does not lie simply between non mental objects (things) and thoughts There is a *double* distinction viz (1) between things and minds both alike regarded as *objects* and (2) between all objects mental or non mental and the *thoughts* of these objects This is the distinction involved in the cognitive relation object' and mind being the terms related and knowledge 'apprehension' etc the names for the relation Or we may push the subtleties still further and ask, whether we ought not to distinguish between a mind and its thoughts, as well as between its thoughts and the objects of these thoughts even though one of them be the mind itself And whether in the end a mind can think anything at all but itself? All these dialectics and plenty more, are to be found in the literature of the subject and very pretty sport they make for those who care to exercise their ingenuity in this way As the net result for our present purpose we may perhaps carry away this moral that we must distinguish between (*a*) the grounds on which within the totality of possible objects of thought 'mental may be differentiated from non mental objects without any reference to the cognitive relation and (*b*) the ground on which in analysing the cognitive relation of a thought to the object of which it is said to be the thought *any* object whatsoever is discriminated from the thought of that object or from the mind that thinks the object The issue between Realism and Idealism at least as interpreted by Realists arises wholly under (*b*), being based on the cognitive relation The decisive question is whether that relation is essential and constitutive for the existence and nature of all objects whatsoever Most Realists hold that there are objects for which the relation is temporary and accidental and which are therefore, "independent and "real and other objects which are "mental in the special sense that they occur only in this relation In this form the issue involves the logical character of *relations* a problem with which I am not concerned in this paper But an examination of some other arguments concerning the 'mental character of objects will be found below in the "Conclusion In this context the paradoxes about "ideas," alluded to below in the text, should also be noted

then be that for these unreal objects, these creatures of dream and imagination, the Idealistic theory holds good for them *esse est percipi*. On the other hand real objects are thought to have an "independent existence *per se*." They pass into and out of the cognitive relation in principle without modification. The status of being apprehended which they may temporarily occupy makes no essential difference to their character or existence. On this view, then, the total universe of our consciousness—the universe which is co-extensive with the range of mental activity—is made up of real or non-mental and unreal or mental objects, of which the former would survive, and the latter perish, with the disappearance of minds. It is also implied that while the universe of consciousness contains much that is unreal, it is very far from containing all that is real. The real objects which at any one time are within it are only a fragment of a vaster non-mental universe of independent entities.

Very well, let us see whither this view leads. What objects are mental in this sense? Dreams and imaginations we have mentioned already. Hallucinations and illusions are an obvious addition. In general all errors and mistakes and "mere," or "subjective," ideas belong to the mental group. And why only false ideas? Why not all ideas whatsoever? Does not truth, after all, belong to the mind as well as error? Commonsense will hardly object, and there is good philosophical authority from Descartes onwards for treating all ideas as mental. Indeed if an "idea" is not mental, what is? But 'Achtung!' as the tobogganists cry at Swiss winter-sports. What in the world is not idea or capable of becoming idea? For an idea is the thought of an object, and even the most realistically real object can be thought of. It *must*, in fact, be thought of, else how could the Realist theorise about it? It thus can assume the status of an idea. But does it not hereby become 'mental,' an element in some mind? With the disappearance of mind will not then the universe *quâ* idea, *quâ* object of mind, disappear? If you eliminate minds, so it would seem, you eliminate their ideas, and thereby the universe so far as it is "object." But, if so, is there any universe left at all? Once more the Realist, if rashly tempted to commit himself to this line of thought, finds himself in dire peril on the precipitous edge of Idealism. He can save himself only by abandoning the identification of real with non-mental, and of unreal with mental objects, and falling back on the broad principle that "activities"[11] of apprehending are mental, and that nothing else is. All objects, therefore, will be real *quâ* objects. We return, in short, with fresh appreciation to the *cognitive* sense of these terms, the recognition of which, as I said above, is the characteristically new feature in Neo-Realism.

The ambiguity of the terms "mental" and "real," and the instability of the current distinctions between them, may be illustrated by another line of thought Among the objects which are mental, and which would disappear with the disappearance of minds, there are, according to a well-known view, all colours and sounds, all tastes, smells, touch-qualities, temperature-qualities, in short, what philosophers have called the "*secondary*" qualities of things. The argument in support of this view is almost too familiar to need re-statement. Real and non-mental are the physical disturbances, the air-waves, the ether-oscillations, the chemical processes in nose and tongue; real also are the effects of these agencies on the nervous system— the nerve impulses started in the sense-organs and propagated to the brain. Only their ultimate results, the "sensations" of colour, etc., are mental. A pleasing, but in spite of much scientific support, alas, an unstable view. For, clearly, from the point of view of knowing, we have to begin with these mental objects, and by an effort of inference we have to reach the non-mental causes that lie beyond them. These are not, therefore, directly apprehended. They are matters of inference, that is of *theory;* they are assumed as *hypotheses,* whose maintenance depends on the somewhat precarious factor of their "working" value. No doubt, many scientists, being--perhaps excusably- little interested in the philosophical aspects of their procedure, take atoms, and electrons, and electric vortexes, and ethers and rays of all sorts, as so many solid facts. But those with a taste for speculation soon realise the hypothetical, the often almost fictitious, character of these entities. I need not quote Helmholtz, Mach, and other leading authorities, in support of a view which I may assume to be familiar The more science goes beyond the direct matter of fact, presented by the senses, and builds up its theoretical edifices with hypothetical bricks, the more does it deal with "things of the mind." The universe which is "real" in the naive sense, cannot well, it is thought, be the abode of things which are fictions and whose existence depends on the maintenance of hypotheses. If the objects of all imagination are mental, and therefore unreal, how can the objects of the scientific imagination be exempt? But behold the paradoxical conclusion to which we are driven. The world of the physicists, so far from being through and through "real" and non-mental, is through and through mental and "unreal." For whether we take it as colours and sounds, etc., or as electrons and energies, in either case arguments for the "mental" character of their universe can be brought forward, which scientists themselves have endorsed. There are only two remedies: either to revise the whole loose conception of "mind" and the loose distinction of "mental" and "non-mental," and the loose identification of this pair of terms with "unreal" and "real" respectively,[12] or to cease regarding the "mental" character of objects as fatal to their "reality." In a sense, these alternatives are not exclusive, but, *prima facie*, Realism adopts the first and Idealism the second.

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[11] "Activities" is used in a quite general sense, being a term employed by all *schools of Neo-Realism. But among themselves they differ* widely as to the character of the cognitive activity. To some American Realists it is apparently identical with the selective reaction of a nervous system to stimuli. Professor Alexander treats knowing, thinking, perceiving, etc., as so many "conations." And to him, as to most English Realists, these activities are ultimate, unanalysable modes of being conscious, modes of awareness. Similarly, "relation," as applied to knowledge, is a word common to all Realists, though the nature of the "*cognitive relation*" is very differently interpreted by different schools. On both these points see below, Part II. of this paper.

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[12] Some further observations on these points will be found in the "Conclusion" of this paper.

We may, perhaps, summarise the net result of this first and longest part of our argument by saying, (1) that we have found all the current distinctions between reality and mind, however convenient in their own context, to break down under the pressure of philosophical criticism; (2) that we had better say, with the Realists, that any and every object of apprehension has "reality" or "being" in the most fundamental logical sense of "objectivity" or "Gegenstandlichkeit"; (3) that, on this common basis, the issue between Realism and Idealism is whether, over and above this sense of reality, there is a further sense in which all objects (or perhaps only some of them) are "independent" of apprehension and therefore of mind,[11] and what is meant by "mind" in this context.

PART II. NEO-REALISTIC THEORIES OF MIND.

The general survey, undertaken for the sake of orientation, which we have just completed, will now enable us to appreciate the exact bearing of the Theories of Mind, offered by modern Neo-Realists. We must say "Theories," for it is just on this central point that Neo-Realists differ most strikingly from one another. In order to keep this paper within reasonable limits, and not to blur the points which I shall try to make, I shall neglect all minor variations and go straight to the fundamental cleavage which ranges the English and the American Realists in opposite camps. To state their difference at once in technical language: the English Realists are *Dualists*, the American Realists profess an "Epistemological *Monism*." In other words, for the English schools *minds or consciousnesses are a distinct kind*

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[11] This is, of course, the same issue as that mentioned at the end of the footnote on p. 168, viz., whether there are objects which are "real" in the sense of having an existence apart from their occurrence as terms in cognitive relation.

of entities or things *subject*-reals, if I may use the word, as against all other entities which are *object*-reals. They hold that there is something distinct from all objects, having (as it were) a substantial existence of its own, and this something, the activities of which we call sensation, perception, thought, memory, imagination, etc., is mind or consciousness. The Americans, on the other hand, hold what they describe as the *relational theory* of consciousness, a theory at first sight much more difficult and paradoxical than the English theory, yet containing, as I believe, the truer principle. On this theory, which has as yet been stated only in fragments, *a mind is simply a selected group or class out of the total universe of things which subsist*. My mind, *e.g.*, at any given moment is just that limited range of objects, whether perceived or thought or imagined, etc., which at that moment lie within, or constitute the field of my consciousness. "Consciousness is merely a demonstrative term, a field of consciousness is just an inclusive phrase for that fragment of the universe which is at any given moment an object." It means the relation or principle of grouping which, from moment to moment, out of the totality of reals in the universe, brings just this miscellaneous lot of odds and ends together which I see, hear, think, doubt, etc., etc. The explanation of the grouping is to be found in the selective action of the nervous system which responds only to certain elements in the total environment.

Let us trace the difference between these two views more in detail.

The English Realists (we may take Professor Alexander as a clear example of this line of analysis) start from the "cognitive situation," *e.g.* I perceive a tree. Direct inspection of this situation reveals the fact that we have here the "compresence" or "togetherness" of two things, viz., a physical thing, the tree, and a mental thing, viz., the perception or act of perceiving. The Dualism of this view is obvious. The function of the perception is to reveal the tree just as it is, hence it is, or ought to be, infallible, there ought to be no room for any mistake: it can not falsify or distort. For consciousness is a quality-less transparent medium which, having no character of its own, cannot alter or modify the character of that which it reveals. When steeped into this medium, as it were, things are said to be apprehended; but it is not necessary to their existence or to their nature that they should be so steeped. They are what they are whether or no consciousness envelops them wholly or partially for a longer or a shorter time. Only in one respect does Professor Alexander indicate the character of these quality-less transparent acts of consciousness a little more fully. He describes them as "conations,"[14] with reference, it would seem, chiefly to the element of attention to or direction upon an object. A mind thus is a system of conations. In part the point of this view is polemical and directed against much current Psychology which, by means of the ambiguous phrase "content of mental processes," places within the mind all kinds of elements which, like the so-called "qualities of sensations" and "images" and "ideas," are for Professor Alexander part of the objective reality strictly to be distinguished from the mental processes or acts which are transparent conations directed upon these "objects."

Now this view is not nearly itself so transparent as it looks. We can ask several awkward questions about it.

(1) In the first place, if consciousness has no character of its own, how can we recognise it or identify it at all? How can we distinguish it, when present, from the thing which is its object? What is the difference, except in words, between the tree by itself and the tree enveloped in this translucent medium? In short, does the word "consciousness" stand for anything at all?

(2) If consciousness has no character of its own, how can we distinguish between different forms or modes of it? What is the difference, *e.g.*, between perceiving and thinking and imagining? Clearly there is only one way out—

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[14] See his paper in the *British Journal of Psychology*, vol. iv, 1911, entitled "Sketch plan of a Conational Psychology."

the way which Professor Alexander is compelled to take — viz., to connect apparent differences of consciousness with genuine, qualitative differences of its objects, to correlate sensing with "sensa," perceiving with "percepta," thinking with "cogitata," and so on But this, I submit, may save the situation verbally, but is in effect a confession of the break-down of the attempt to distinguish consciousness as a separate "somewhat" from its "objects."

(3) Suppose we grant to Professor Alexander that every item of perception or thought or memory, etc., is in itself directly apprehended, such as it is, there yet remain to be accounted for all the operations of synthesis and analysis, of interpretation, of inference in short, all that is usually described as the work of "constructive intelligence." For example, I do not simply "see" a tree On the contrary, visual perception, stripped of all inferential interpretation and reduced to the actual visual datum, presents me merely with a variously coloured patch of a certain size and shape in my visual field. That this, which is all I strictly "see," is or means a tree, is more than sight, it is inference. But, supposing it were all sight, I should not ordinarily stop there, but go on to think about the tree, to recall other things connected with it or bearing on it, *e.g.*, that it illustrates certain botanical principles, or that I climbed it as a boy, or that it is to be cut down for fire-wood. The chief function of what is ordinarily called "mental activity" is just this expansion and amplification of the "given" object. We do not merely apprehend passively, but we elaborate actively. We add, we modify, we analyse, we interpret. True, it is through these processes that error is apt to enter, but it also is no less true that only through these processes do we achieve knowledge. How can the theory of a qualityless activity account for these facts? A conation, such as Professor Alexander describes, might conceivably be directed upon a given something. But how could it add to it? Or go beyond it by memory or inference or imagination? Clearly, the nexus which makes the additions rele-

vant, and the whole spring of the movement which leads to the additions, is to be sought *in the nature of the objects*, not in that of the supposed transparent conations.[15]

In principle the same distinction between an object and a transparent mental act is drawn by Mr. G. E. Moore, of the Cambridge School of Realism. To reduce the problem to its most elementary form, he takes the example of a "sensation of blue,"[16] which he analyses into the object-element "blue," the mental element sensation, and a unique and not otherwise describable relation between these two. The sensation is "a case of 'knowing,' or 'being aware of,' or 'experiencing' something" The blue is not a mental "image" or the quality of anything mental at all. It is a real, independent feature of the universe, standing momentarily in this unique relation to a sensation or act of sensing. To reflect on one's sensation of blue is to be "aware of an awareness of blue." Mr. Moore goes on to point out that there is this difficulty about the analysis, *viz.*, that to fix one's attention on the "consciousness" apart from the "object," is apparently to focus on mere emptiness, on something so "diaphanous" that nothing seems to be there at all. Most students will agree with Mr. Moore in this, but they will draw the opposite conclusion from that which Mr. Moore has drawn They will conclude that consciousness is not distinguishable, in the sense required, from what are called its "objects." But Mr Moore sets out with the determination to find such a distinction, or if he cannot find it, then to manufacture it in the name of Realism. Thus he is driven to invent a diaphanous consciousness to fill the gap. But there is, as I shall try to show below, another way.[17] Meanwhile, suffice it to say that Mr. Moore has not, so far as I know, extended his analysis to other modes of mental activity. Hence, whilst his view is open, in principle, to the same objections as Professor Alexander's, we have even fewer data for inferring how Mr. Moore would deal with the difficulties.

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[15] This is really the feature on which "Objective Idealism" insists. See below in the "Conclusion" of this paper.

[16] See Mr Moore's article, *A Refutation of Idealism*, in *Mind*, N S., No. 48 (Oct., 1903), p. 449

[17] See the "Conclusion" of this paper.

Now, the American Realists, as I said above, recognise no such mysterious, diaphanous "consciousness" existing alongside of independent "objects." They take their cue from an article, written by William James in the last years of his life, which is entitled: "Does Consciousness Exist?"[18] It is interesting to note here that, whilst never abandoning Pragmatism, James was the author of articles which make him unmistakeably the forerunner of American Realism. In the paper just mentioned he says of "experience" that "it is made of *that*, of just what appears, of space, of intensity, of flatness, of brownness, of heaviness, of what not . . . . Experience is only a collective name for all these sensible natures. . . ." In short, he unmistakeably identifies experience with the objective context, with the world, the universe. Now this is just what "objective" Idealism also does. Perhaps the point becomes clearest by asking: what, after all, is the character of seeing as distinct from the colours seen, of hearing as distinct from the sounds heard, of thinking as distinct from thoughts, *i. e.*, from what we think, of consciousness or awareness as distinct from what we call its objects? James replies: There is no such distinction. Seeing means colours occurring, hearing means sounds occurring, thinking means thoughts occurring all these in a certain context, described as a "field of consciousness." We have, in short, the relational theory of consciousness: a mind is a selection, a fragment out of the total mass of "being." On this basis, a Realistic theory can easily be built up. We need merely assume that the selection or grouping is, in a manner, accidental to the elements thus brought together, that these elements have a nature of their own, and stand in relations to one another, and to other elements not included in the selection, which nature and relations are independent of this special grouping, so that they may be taken to exist, whether or no they happen to be grouped together as a mind— we need merely assume this to get straightway a realistic conclusion.

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[18] See *Essays in Radical Empiricism*, pp. 26-27.

If my account is sketchy, I must excuse myself with the fragmentary and tentative statements of my authorities. The most important of these are Professors Perry and Holt, of Harvard, who both, as I read them, hold such a view as I have attempted to outline, except that Perry appears to lay undue stress on the physical environment as the totality, out of which the response of a nervous system selects the items which, at any given moment, constitute a "mind." This seems to me unduly narrow, to lay too much emphasis on objects of perception, and to neglect the ranges of objects with which we deal in Mathematics, Logic, Ethics, Religion and other branches of thought. Holt, more soundly, makes his "universe of being," within which minds are subordinate groups, all-inclusive.[19]

By way of rounding off this sketch of the Neo-Realistic theories of mind, let us once more apply our test-question. We then find: (1) That for English Realists the world would by the disappearance of minds lose only a transparent something-or-other which leaves the rest exactly as it is; and (2) that for American Realists there disappears only a special grouping or relationship of the objective elements of the

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[19] Perry (see, *e.g.*, *Present Philosophical Tendencies*, ch. xiii, §10) seems to take a too narrowly biological view in treating consciousness as a "species of function" exercised by an organism upon an environment. This tends to restrict the environment to physical, *i.e.*, ultimately to material, nature. At any rate, there is no hint of the environment being, *e.g.*, moral or social or æsthetic or religious, and of the organism reacting to these characters as well Holt (*New Realism*, p. 372), on the other hand, says "The picture I wish to leave is of a general universe of being in which all things physical, mental, and logical, propositions and terms, existent and non-existent, false and true, good and evil, real and unreal *subsist* . . . . . A mind or consciousness is a class or group of entities within the subsisting universe, as a physical object is another class or group." The whole passage should be read it puts the important points with great clearness.

I ought to add that, in singling out Professors Perry and Holt as the spokesmen of American Realism on the subject of mind, I am omitting Professor Montague's view (*New Realism*, p. 281). Not feeling sure that, in its present form, I understand this view, I think it safer not to comment on it universe. The universe, composed in the last analysis of simple, unanalysable entities, would remain what it is, notwithstanding the elimination of minds. It would only cease to be carved up, here and there, into a special kind of temporary aggregations of selected elements On either view, reality in its own character is indifferent to the presence or absence of minds—that is what makes these views realistic.

CONCLUSION: THE MAIN POINT FROM THE REPLY OF IDEALISM.

Throughout this paper I have had occasion to emphasise the issue between Realism and Idealism. Hence it will not be inopportune if I conclude by outlining, briefly, the main point of the Idealistic theory concerning the nature of mind.

I said above that Idealism construes the universe, in last analysis, as a mind, the so-called Absolute Mind, or as a system of minds Fortunately, it is not necessary for our present purpose to go into the difference between these two the Absolutist and the Pluralist– views. Questions of considerable technical difficulty are involved which lie outside the proper limits of this paper. It will suffice if we concentrate on the principle which is common to all forms and varieties of Idealism, viz., the identification of reality in its full character with the nature and life of mind.

Now, thus boldly stated, this theory may easily seem the wildest of paradoxes, but the paradox soon disappears when we frankly and fairly face the issue: what does "mind" mean? What features of the universe does the term stand for or point to? And are these features so fundamental that we have a right to acclaim the universe, in its final constitution, as mind?

This problem is purely one of *Logic*—of Logic in the sense in which its task is to trace out the conceptual structure of the universe, or, in other words, the structure which the universe must have, on the assumption that it is a single self-consistent whole. In the technical language of Idealism, it is a problem of the adequacy of certain "categories" for metaphysical theory. All difficulties and misunderstandings which divide Idealists and their critics arise from the fact that the meaning of mind is ambiguous, that the word is variously used to point to features of the universe which stand on widely different levels in the scale of categories, which are, in other words, of very different degrees of adequacy to the nature of reality *as a self-consistent all-inclusive whole.* I will point, in support of this contention, only to two meanings of "mind," neither of which is that of the Idealists, but each of which has been attributed to them by their critics, and made the ground for a complete rejection of their theories.

(1) The first is the *psychological* theory of mind as a flux of perceptions and thoughts—the "stream of consciousness," in William James's picturesque phrase. This theory may be adequate for Psychology, but it is agreed on all hands that it is inadequate as the basis for a theory of the universe. Its category is that of ceaseless, unstable, temporal sequence. The mind is the variegated sequence of all the feelings, sensations, ideas, memories, etc., that follow one another in rapid change from moment to moment No one can consistently suppose—though Hume tried to do so—that this flux is the last word, the ultimate truth, about the reality which we claim to perceive, think, recollect, etc. To have shown the inadequacy of this view is the achievement of Kant in his reply to Hume. And Kant succeeded because he took his stand on the nature of *judgment*, and through it on the *logical structure* of our universe which is expressed and affirmed in our judgments.

(2) The second misleading interpretation of mind is one which, borrowing Professor Perry's convenient phrase, we may call the *ego-centric*.[20] The term calls attention to the fact to which all the language inevitably used by us bears witness, viz., that minds, at any rate in their mature human

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[20] See *Present Philosophical Tendencies*, ch. vi., §10, p. 129, on the Ego-centric Predicament.

form, are *selves*. Each of us says: *I* think, *I* feel, *I* will, etc. Each of us speaks of *my* perceptions, *my* ideas, *my* world. There is no object, no element, in the universe which, as perceived, thought, spoken of, does not thus come within the "field of consciousness" of a thinker, a self, an Ego. But, argue the Realists, these temporary associations of real objects with Egos (= English Realists), these temporary aggregations of real objects into ego-centric fields of consciousness (= American Realists), are not the ultimate truth about the universe. Minds, as Selves, are transitory forms. The range of reality which they cover, whether at any given moment or in the ideal sum of the moments of their lives, is a mere fragment of the infinite totality. The parts of reality that a Self at any time apprehends, are mere isolated, selected, abstracted bits of the whole, and these aggregations are unstable and constantly dissolved and re-constituted. Again we see that the criticism, in its fundamental logical motive, turns on the inadequacy of the categories employed. There is again the challenge on the ground of the transitoriness and instability which is inseparable from *time;* there is the challenge to the inadequacy of *fragments* isolated from the *whole*, there is the challenge to the largely *chaotic* and *accidental* nature of these fragmentary selections from the whole, as against the *orderly* and *systematic* character of the whole; there is, lastly, the challenge to the *dispersion of reality* into an indefinite multitude of finite minds, which between them may not cover the whole.

And so, on all these grounds, the Realists reject the Idealistic identification of reality and mind: they labour to discriminate between the nature of reality and the nature of mind. But, in all this, they are really fighting for a *logically adequate* conception of reality: they are fighting against inadequate categories. And, therefore, they are in the paradoxical position that, *at bottom, they are fighting, not against Idealism, but with it.*

For the Idealists, as I understand their theory, entirely and whole-heartedly agree with the Realists' criticisms of mind understood in either of the above-mentioned ways. *In fact, they have urged these very arguments themselves.* But they reply that the Realistic theories overlook altogether certain all-important features of the universe, and that it is just these for which Idealists use the term "mind."

Perhaps I can best make this Idealistic conception of "mind" clear by saying at once that the Idealist has no use for the English Realists' conception of an indescribable, diaphanous consciousness. On the other hand, he can make some use of the American Realists' conception of a mind as a selection, and therefore a fragment, out of a wider objective totality. For this is exactly what the Idealist calls a "finite mind." But the Idealist does not stop at this point. For if this were the whole truth about our minds, we could not possibly know it. If my mind were nothing more than a selection out of the totality of real objects which compose the universe, I should never be able to discover the fact, unless I were in some way aware of this very totality *from* which I select, of the *ranges of fact* lying beyond the circle of my selection. We have, in short, to think the whole, in order to distinguish ourselves as parts within it. And, in order to *think* the whole of which it is a part, a mind has, in the measure of the completeness of its thought, to *be* the whole. If our minds are selections out of the totality of reality and, at the same time, recognise themselves as selections, then in this recognition they affirm themselves to be the whole, focussed in fragmentary form. A mind, which *ex hypothesi* were no more than a fragment, could not recognise itself as such, and therefore could not distinguish itself from, or within, the whole. This is the paradox of the "logic of self-consciousness"—yet it does no more than point out the logical nexus of our thought in this act of distinction and recognition of the self within its world. It is also the paradox of what Idealists of the Hegelian school have called the "finite-infinite" nature of mind, which is nothing but an attempt to formulate the plain logical situation which consists in a mind, conceived as a fragment of reality, differentiating itself from the whole reality, and yet at the same time acknowledging itself as a part of that reality. To do that, it must be more than "finite": it must be a "finite" form of the "infinite."

Now, whatever we may think of this paradox when thus baldly formulated, it is at any rate possible to point out some features of our universe which support it. These features are *logical*, and are revealed in the transformation which the real, as given, undergoes (in the form of what we call judgment, inference, science, knowledge) under the pressure of the demand for consistency—a demand which is an ideal to be achieved just because it expresses the inherent and fundamental character of the real in its "true," *i.e.*, logically stable, form. The whole drama of the universe, we may say paradoxically, is thus in its core a struggle towards logical completeness. The fragments, from the friction and contradiction of their isolation, strain back to the self-consistent completeness of the whole. It is this *movement or nisus towards logical stability* which runs through all that we perceive and think. These terms, perception and thought, stand, as we have seen, for no bare, featureless activity or consciousness; they stand for the "objects," for *what* is perceived and thought *and* (we must now add) for the ceaseless processes of supplementation, combination, differentiation, interpretation, "construction," which transform any given elements in the direction towards a logically stable form. Take the world at the beginning and at the end of a process of thought, say the solar system as it was conceived by primitive man, by the Ptolemaic astronomers, by Kopernikus, by twentieth century science it is the *same* in one sense throughout, yet what a transformation! It is not merely that we know more about it, but that we have re-arranged it all. I say "we" have re-arranged it, but the nature of the facts demanded it. They re-arranged themselves. In fact, however, paradoxical the language, it makes little difference whether we say: we think the world so-and-so, or: the world thinks itself so-

and-so in us. As Spinoza would say: "deus sive natura, quatenus humanam mentem constituit."

In conclusion, I will summarise the positions which I have tried to maintain in this paper. In Part I., in addition to a general survey for the sake of orientation, I showed (i) that the New Realism does not, like the Old, conceive reality as exclusively material, but tends to think of it as comprising all objects of any kind which can in any way be experienced or apprehended; and (ii) that it thus starts from an analysis of the cognitive relation between mind and reality, and that the antithesis of these two terms dominates Realistic thought throughout [21]

In Part II. I contrasted the English and the American Realists' theories of the nature of mind or consciousness, and showed (i) that the former tend to think of mind as a transparent something, distinct from the object, though directed upon it: and (ii) that the latter identify mind with a selection from the totality of what is objectively real.

In the Conclusion I argued (i) that, whilst the theory of the English Realists has no common ground with the Idealistic theory of mind, the theory of the American Realists offers a basis for discussion For the identification of mind with the objective field of what is perceived and thought, and the recognition of this field as a fragment of a wider whole is common to both Idealism and American Realism. But

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[21] It should be noted in this connection that, for reasons both of space and of clearness, I refrained from touching on all those arguments of the Neo-Realistic schools which are built up on the axiom that relations are external to their terms, and that these terms are ultimately simple, unique, and unanalysable. All Neo-Realists seem to be agreed on this axiom of "External Relations," though some make more use of it than others The questions involved are exceedingly technical and difficult, and at bottom the issue lies between two Theories of Logic, the Idealistic Theory on the one side, and modern developments of Symbolic Logic on the other Thus the problem of relations is another point of conflict between Idealism and Neo-Realism. In fact, they give battle to one another on a good many points, of which the problem of mind is only one, though one of the most important It is clear that, if the principle of External Relations were established beyond dispute, and if it were also established that knowledge is correctly construed as a relation, then the Realistic Theory of the independence of reality on its relation to minds which know it, would follow by a simple syllogism.

(ii) I tried to show that both forms of Realism fail to take account of the one feature of reality which, for Idealists, is more fundamental than any other, viz., the logical striving of reality, in the form of finite minds, from its fragmentariness in this form towards supplementation and self-completion. Hence the "true" nature of fact is not to be found except in terms of stable and logically consistent theory; all perceiving and thinking, with their progressive stages of intellectual interpretation and construction, are but the self-revelation and self-transformation of the real from more fragmentary and less stable to less fragmentary and more stable forms. The details of this view have to be, and have been, worked out in systems of logic, which, guided throughout by the clue of self-consistency or logical stability, trace in detail the structure of the real world of our knowledge.

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THE OXIDISING PROPERTIES OF SULPHUR DIOXIDE.

By J. A. SMYTHE, Ph.D., D.Sc., and W. WARDLAW, B.Sc.

Sulphur dioxide is so well characterised as a reducing agent and so extensively applied for purposes of reduction, both in the laboratory and in technical operations, that its oxidising properties have been somewhat neglected.[1] It is sufficient to compare the text-books of thirty years ago with their modern substitutes to appreciate that this aspect of the compound, though not quite forgotten, has been to a large extent overlooked. In the literature of the subject, however, many reactions are recorded in which sulphur dioxide plays the part of an oxidising agent, and a brief account of these reactions may be given before the narration of our own experiments on the subject.

Combustion Reactions.—Many elementary substances, especially when finely divided and strongly heated, burn in the gas. Potassium, tin, iron, lead, arsenic and antimony are mentioned in this connexion, and to these may be added magnesium and calcium, which burn with extreme brilliancy. The products of reactions are oxides and sulphides and, occasionally, sulphur is set free.

Reaction with Hydrogen Iodide.—Though sulphur dioxide reduces iodine quantitatively in dilute solution, the reverse action takes place in solutions of very moderate concentration (above 0.05 per cent. of acid). This fact, of fundamental importance in analytical chemistry, was first established by Bunsen.[2] In a more concentrated state, sulphur dioxide is able to oxidise hydriodic acid to iodine, being itself reduced to hydrogen sulphide. These reactions are of especial interest as typical of many to be described later.

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[1] It may be of interest to note that sulphur dioxide, SO_2, like its formal analogue ozone, OO_2, exhibits both oxidising and reducing characters.

Reaction with Stannous Chloride. This remarkable reaction, whereby the sulphides of tin are precipitated by sulphur dioxide, finds frequent mention in the older books and will be referred to in detail later.

Reaction of Zinc and Sulphurous Acid.—The reduction of sulphur dioxide solution with zinc, and other metals, in presence of hydrogen chloride has long been known and used as a delicate test for the compound. An important modification of the reaction is that in which the metal is digested with sulphurous acid alone; no hydrogen or hydrogen sulphide is then evolved, but a strongly reducing solution is obtained. This was noted by Schönbein in 1852 and led, in 1869, to the discovery of hyposulphurous acid, $H_2S_2O_4$, by Schützenberger. The reaction was studied in detail by Bernthsen and Bazlen,[3] who first prepared and analysed the pure salts of the acid, and it was found later that the same acid results by reduction with titanous chloride.[4]

Reaction with Hydrogen Sulphide.—This is one of the few reactions mentioned specifically in the text-books as showing the oxidising properties of sulphur dioxide. It has been extensively studied, especially with a view to the preparation of the polythionic acids.[5] These are regarded by Debus as intermediate between sulphur dioxide and hydrogen sulphide, and the complete reaction between these compounds is expressed quantitatively by the equation:—

$$2\,SH_2 + SO_2 = 3\,S + 2\,H_2O.$$

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[2] *Annalen*, 1853, lxxxvi., 261; also *Gesammelte Abhandlungen*, vol. ii. p. 196.

[3] *Ber.*, 1900, xxxiii., 126.

[4] E. Knecht and E. Hibbert. "New Reduction Methods in Volumetric Analysis," 1912, p. 5.

[5] See, in particular, H. Debus. *Soc.*, 1888, liii., 278, where references to the earlier literature are given.

The influence of the solvent on this reaction has also been investigated,[6] with the result that no connexion has been found between reactivity and the dielectric constant or association-factor of the solvent; those media promote action in which the formation of an intermediate compound with either sulphur dioxide or hydrogen sulphide is possible.

The reactions of the organic analogues of sulphur dioxide and hydrogen sulphide, viz.: the sulphoxides and mercaptans, have been studied in some detail, during recent years, with results germane to the present subject. In fact, it was the recognition of the oxidising properties of the sulphoxides,[7] first clearly enunciated by Hermann, and the proof that sulphur dioxide can oxidise mercaptans,[8] which directed the attention of the authors to the oxidising properties of sulphur dioxide with respect to inorganic compounds. The mutual relations of sulphur dioxide and hydrogen sulphide and of their organic derivatives, the sulphoxides and mercaptans, may be illustrated by the three following (quantitative) reactions:[8]

$$SO_2 + 4\ CH_3\ C_6H_4\ SH = (CH_3\ C_6H_4)_2\ S_2 + (CH_3\ C_6H_4)_2\ S_3 + 2\ H_2O.$$

$$(CH_3\ C_6H_4)_2\ S_2O_2 + 4\ CH_3\ C_6H_4\ SH = 3\ (CH_3\ C_6H_4)_2\ S_2 + 2\ H_2O.$$

$$(CH_3\ C_6H_4)_2\ S_2O_2 + 2\ SH_2 = (CH_3\ C_6H_4)_2\ S_2 + S_2 + 2\ H_2O.$$

It will be noted that, in all cases, the sulphur dioxide and derived sulphoxide act as oxidising agents, the hydrogen sulphide and derived mercaptan as reducing agents.

The Reduction-Products of Sulphur Dioxide. When sulphur dioxide exercises an oxidising action it is itself reduced and the possible reduction-products are lower oxides of sulphur, of the general formula $S_n\ O_{2n-m}$, sulphur and hydrogen sulphide. Of these lower oxides, only one, S_2O_3, is known and it is the anhydride, formally speaking, of hyposulphurous acid, though the relationship could not be proved, experimentally, by Bernthsen and Bazlen (*op. cit.*). All the oxy-acids of sulphur, except dithionic acid, which is an oxidation-product of sulphur dioxide, may be regarded as derived from these lower oxides by combination with water, and corresponding to this, they are all prepared by reduction of sulphurous acid or sulphites with mild reducing agents (sulphur, hydrogen sulphide, etc.), and, generally, two or more of the acids are formed in the same reaction.

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[6] D. Klein. *Journ. Phys. Chem.*, 1911, xv., No. 1, p. 1.

[7] F. Hermann. *Ber.*, 1905, xxxviii., 2824; 1906, xxxix. 3812.
J. A. Smythe. *Soc.*, 1909, xcv., 349; 1914, cv., 546.
B. Holmberg. *Ber.*, 1910, xliii., 226.

[8] J. A Smythe and A. Forster, *Soc.*, 1910, xcvii., 1195.

Some examples of the reduction of sulphur dioxide to sulphur and hydrogen sulphide have already been given and our experiments have been chiefly directed towards this aspect of the subject, the reducing agents employed being the lower chlorides of the metals So far as we have been able to determine, only one such reaction, that with stannous chloride, has been described before, and the published accounts are lacking in precision.

It will be obvious that opportunity for reaction occurs with the lower chlorides, which may become oxidised, in presence of hydrogen chloride, to the higher chlorides, with simultaneous production of sulphur or hydrogen sulphide, or, in favourable cases, precipitation of the metal as sulphide owing to secondary action.

Not all of the lower chlorides of the metals are oxidised in this manner. We have been unable to get any evidence of reaction in the case of arsenic, antimony, lead and thallium. The lower chlorides of chromium and molybdenum, on the other hand, react with great ease, reducing the sulphur dioxide to hydrogen sulphide and being themselves oxidised to the higher chlorides. Cuprous chloride reacts to a very slight extent with production of sulphur and cuprous sulphide, but the precise conditions necessary for this reaction to take place are obscure and have not yet been fully worked out. Four cases have been studied in some detail and will now be described.

OXIDATION OF METALLIC CHLORIDES WITH SULPHUR DIOXIDE.

Stannous Chloride.—Most of the older books mention this reaction, some (Mendeléeff, Miller), stating that stannic sulphide, others (Odling, Watts), that stannous sulphide is precipitated. The difference in behaviour towards sulphur dioxide of the chlorides of tin, arsenic and antimony has been utilised in order to detect the last two metals in presence of excess of tin,[9] and the oxidation of stannous chloride with sulphur dioxide has been suggested as a preliminary step in the separation of the metals of Group II. in the analytical classification.[10]

The reaction takes place with ease in warm, acid solution and it is evident, from the colour of the precipitate, that stannous sulphide is first formed; the colour, acid solution and it is evident, from the colour of the precipitate, that stannous sulphide is first formed; the colour, however, changes quickly to the yellow of the stannic sulphide, undoubtedly mixed with sulphur. In highly acid solutions the sulphides are not precipitated but hydrogen sulphide is evolved. This reaction can be well shown by adding a crystal of sodium sulphite to a warm, strongly acid solution of stannous chloride contained in a test tube; the hydrogen sulphide comes off so vigorously that it may be burned at the mouth of the tube The equation representing the reaction is:

$$SO_2 + 6\,HCl + 3\,SnCl_2 = 3\,SnCl_4 + 2\,H_2O + SH_2$$

The complexity of the reaction is apparent from this equation, for the ratio of stannous to stannic salt rapidly diminishes as reaction proceeds and (in moderately acid solutions) the two salts, as well as the sulphur dioxide, enter into competition for the hydrogen sulphide. These facts suffice to account for the progressive change in colour and composition of the precipitate formed under favourable conditions of acidity.

[9] E. Donath. *Zeit. Anal. Chem.*, 1897, xxxvi., 663.

[10] L. J. Curtman and J. K. Marcus. *Journ. Amer. Chem. Soc.*, 1914, xxxvi., 1191.

By operating in strongly acid solutions and passing the hydrogen sulphide, which escapes secondary reaction with sulphur dioxide in the tin solution into a solution of hydrogen chloride saturated with sulphur dioxide, it is possible to study this reaction quantitatively The hydrogen sulphide and sulphur dioxide react according to the equation —[11]

$$2\,SH_2 + SO_2 = 3\,S + 2\,H_2O$$

so that the complete equation of reaction becomes —

$$3\,SO_2 + 12\,HCl + 6\,SnCl_2 = 6\,SnCl_4 + 6\,H_2O + 3\,S$$

The details of the method used are as follows A concentrated solution of stannous chloride is made from tin and hydrochloric acid and its strength determined by means of standard iodine solution An aliquot portion of this is diluted with its own volume of hydrochloric acid (conc), warmed, and then sulphur dioxide passed through for two hours the issuing gases bubbling through two or three absorption vessels containing the sulphur dioxide solution Finally, the sulphur dioxide in the decomposition vessel is expelled by carbon dioxide, the unaltered stannous salt estimated if present, by titration with standard iodine, and the sulphur collected from the decomposition, and absorption-vessels, washed, dried and weighed The sulphur is afterwards identified by recrystallisation from chloroform and the determination of its melting point The results of two experiments, in which oxidation of the stannous chloride was complete may be quoted —

1 25 c c stannous chloride solution, containing 2 89 grams tin gave 0 350 gram sulphur

2 25 c c stannous chloride solution, containing 2 89 grams tin gave 0 371 gram sulphur

Weight of sulphur equivalent to 2 89 grams tin, calculated from equation = 0 389 gram

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[11] H Debus, *loc cit*

Titanous Chloride—The reduction of sulphurous to hyposulphurous acid by titanous chloride has already been noted and mention though without details, of further reduction to sulphur is made in the literature[12] Our observations point to the sulphur as a secondary product of reaction between the hydrogen sulphide formed and the sulphur dioxide used in the experiment When sulphur dioxide is passed into a warm, strongly acid solution of titanous chloride, hydrogen sulphide is freely evolved and sulphur is deposited in the solution and along the outlet tubes, the violet colour of the solution deepens, at first becoming almost opaque, then dark-grey and ultimately clear again

The quantitative study of the reaction was carried out in a manner similar to that employed in the case of stannous chloride The solution of titanous chloride was standardised by means of ferrous ammonium sulphate and permanganate solution,[13] and an aliquot portion of this treated as described above Complete oxidation takes place in about three hours The building-up of the equation of reaction may be given here as typical of the other cases

$$2\,SO_2 + 12\,HCl = 2\,SH_2 + 4\,H_2O + 6\,Cl_2$$
$$12\,TiCl_3 + 6\,Cl_2 = 12\,TiCl_4$$
$$2\,SH_2 + SO_2 = 3\,S + 2\,H_2O$$

and the combination of these gives the following equation of reaction, with which the experimental results are compared

$$3\,SO_2 + 12\,HCl + 12\,TiCl_3 = 12\,TiCl_4 + 6\,H_2O + 3\,S$$

Results 1 50 cc solution contained 0 77 gram titanium and gave 0 11 gram of sulphur Calculated, $S = 0\ 130$ gram

2 50 cc solution contained 0 80 gram titanium and gave 0 11 gram of sulphur Calculated, $S = 0\ 133$ gram

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[12] E Knecht *Ber*, 1903, xxxvi., 166

[13] E Knecht and E Hibbert "New Reduction Methods in Volumetric Analysis," p 48.

Mercurous Chloride.—When mercurous chloride is warmed with concentrated hydrochloric acid, it undergoes slow decomposition into mercury and mercuric chloride and the solid darkens owing to the separation of the finely divided metal. In presence of sulphur dioxide, however, no darkening of the mercurous chloride is observed, but sulphur is deposited and mercuric chloride passes into solution; hydrogen sulphide can not be detected in the issuing gases and the solution is free from sulphuric acid. The reaction is slow but proceeds to completion in time, from which it is evident that the reduction of mercuric chloride by sulphur dioxide, which occurs in dilute slightly-acid solution, does not take place in presence of much acid.

The equation representing the oxidation of the mercurous salt by sulphur dioxide is:

$$SO_2 + 4\,HCl + 4HgCl = 4\,HgCl_2 + 2\,H_2O + S$$

and with this the quantitative data are in complete accord.

The method used in studying the reaction quantitatively is as follows: about 20 grams of mercurous chloride and 100 c.c. of concentrated hydrochloric acid are warmed by a small flame and a slow stream of sulphur dioxide passed through the liquid for 6 hours. The residue is filtered, washed, dried and weighed, then extracted with chloroform in a Soxhlet tube and the sulphur recovered by evaporation of the chloroform and weighed. From these data, the yield of sulphur from the oxidation of a known weight of mercurous chloride can be estimated.

Results:

	1. Grams.	2. Grams.
Mercurous chloride taken	20·641	21·315
Mercurous chloride unchanged ...	4·055	6·177
Sulphur, found	0·525	0·490
Sulphur, calculated	0 540	0·510

The reaction is facilitated by withdrawing, at intervals, a portion of the solution and replacing it with fresh acid.

Ferrous Chloride.—That the presence of much acid prevents the complete reduction of ferric chloride by sulphur dioxide is a fact known to analytical chemists,[14] and, in the light of the above experiences, one might infer that the reverse process, viz., oxidation of the ferrous salt by sulphur dioxide, would be operative in strongly acid solutions. Experiment shows this to be the case. When sulphur dioxide is passed into a strongly acid solution of ferrous chloride, the colour quickly changes to olive green and then reddish-brown and the liquid becomes opalescent owing to the separation of sulphur; after a little time the sulphur agglomerates and settles out in whitish flocks and scales and a faint milkiness appears in the outlet and guard tubes. This is presumably due to the liberation of hydrogen sulphide, though this gas has never been detected; if produced, whether directly or by secondary reaction between sulphur and water,[15] the quantity must be extremely small. These changes occur in cold, but more readily in warm solutions.

On removing the sulphur dioxide by heating the solution in a stream of carbon dioxide, the presence of ferric salt can be easily ascertained by the usual tests; the solution does not contain sulphuric acid, whence it is evident that simultaneous reduction of the ferric chloride does not take place, and further, that the sulphur set free in the oxidation-process is not oxidised by the ferric chloride, though such happens to a large extent in hot, dilute (one per cent.) solution.[16]

For the detailed study of this reaction, the following method was used. Pure electrolytic iron is dissolved in pure, warm, concentrated hydrochloric acid, a stream of carbon dioxide passing continually through the apparatus. The vessel in which solution takes place is made entirely of glass and is connected with two guard tubes, one containing water, the other caustic soda solution; these serve to absorb the excess of sulphur dioxide and hydrochloric acid, to trap the small amount of sulphur which passes over and to preserve the iron solution from oxidation by excluding air After the iron is dissolved, connection is made with the sulphur dioxide syphon and the gas bubbled through for the required time, the vessel being heated by a small flame The sulphur dioxide is eventually displaced by carbon dioxide, the apparatus cooled in a current of the same gas, the iron determined by standard dichromate solution, and the sulphur, if necessary by filtration and direct weighing

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[14] Treadwell and Hall. Quantitative Analysis, p. 484.

[15] J. B. Sanderens. *Soc. Abs.*, 1892, 770.

[16] H. N. Stokes. Bull. U. S. Geol. Survey. No. 186, 1901.

The experimental study of the reaction has disclosed some interesting features, a summary of which will now be given

Rate of Oxidation —Comparative experiments with solutions containing one gram of iron in 50 c c of acid, the treatment with sulphur dioxide lasting from 1 to 7 hours, show that the maximum yield of ferric iron is only about 6 per cent of the total iron present, this value is reached after 3 hours and oxidation is more than half accomplished during the first hour

Limit of Oxidation —The average yield of ferric iron in all the experiments is 7 per cent of the total iron present, the extremes being 4 and 11 6 per cent The yield is the same, other conditions being equal, whether the reaction occurs in hot solution throughout, or the saturation of the ferrous solution with sulphur dioxide is performed in the cold and the excess of the gas quickly boiled off, after keeping a day or two, in a current of carbon dioxide It is slightly increased (to the extent of one per cent) by additional saturation of the liquor with hydrochloric acid gas Increase in the total concentration of the iron lessens the relative yield of ferric salt, thus in three experiments, carried out under similar conditions, with solutions containing 0 99, 5 45 and 22 68 grams of iron per 200 c c, the yields ($Fe''' : Fe$) were respectively, 6 46, 6 23 and 3 31 per cent Initial reaction, as indicated by the appearance of opalescence, occurs more speedily in concentrated than in dilute solutions

Quantitative Aspect of the Reaction —There are two possible reactions, corresponding (*a*) to direct reduction of the sulphur dioxide to sulphur and (*b*) to initial reduction to hydrogen sulphide, followed by decomposition of this gas with sulphur dioxide as, however, both reactions are represented by the same equation —

$$SO_2 + 4\,HCl + 4\,FeCl_2 = 4\,FeCl_3 + 2\,H_2O + S$$

it is evident that the quantitative relations, even if susceptible of accurate determination, would not enable one to decide between the two reactions In point of fact however, the limit of oxidation of the ferrous salt is so soon reached, and hence the amount of sulphur produced is so small, relatively to the volume of solution that its accurate estimation is attended with considerable difficulty Considering this drawback, the results show a very fair agreement with the calculated values, as may be seen from the following examples

	1	2	3
Concentration of ferrous solution	5 893 grams of iron per 100 C C	4 45 grams of iron per 200 C C	6 450 grams of iron per 200 C C
Ferric iron produced	0 683 grams	0 392 grams	0 340 grams
Sulphur, found	0 062 ,,	0 040 ,,	0 035 ,,
Sulphur, calculated	0 098 ,,	0 056 ,,	0 048 ,,

The conclusion to be drawn from these results, and from general observations on the reactions, is that the oxidation of ferrous chloride by sulphur dioxide is represented by reaction (*a*) above, it is possible, also, that the course of reaction (*b*) is followed to a very slight extent

Inhibition of Oxidation by Ferric Chloride —One unexpected feature of these oxidation-results is the limited yield of ferric salt, no matter how long the sulphur dioxide be present with the ferrous chloride under the known conditions of reaction, there is never more than a relatively small amount of ferrous salt oxidised In the numerous experiments we have made on this subject, the concentration of the ferrous iron has varied from 0 5 to 10 0 grams per 100 c c, while that of the ferric iron, after reaction, has not exceeded

0 6 gram per 100 c c , but has been as low as 0 03 gram per 100 c c The factor of chief influence in limiting the oxidation is evidently not the concentration of ferric iron but the ratio of ferric to total iron (Fe'' Fe) which as stated above, is usually about 6 per cent , but may rise to about twice this value under conditions not yet fully understood These facts suggest that the oxidation of the ferrous salt is inhibited by the ferric salt formed in the reaction and this inference is confirmed in a striking manner by experiments with mixed ferrous and ferric salts An example may be quoted in illustration

Two solutions A and B were made up with the following concentrations —

	A	B
Fe per 100 C C	1 708 grams	1 736 grams
Fe ,,	0 268 ,,	0 292 ,,
Ratio, Fe Fe	13 56 ,	14 40 per cent

50 c c of each solution were warmed to about 60° C and treated with sulphur dioxide for four hours In solution A, a very small amount of sulphur was deposited and subsequent analysis showed that only 4 mgms of iron had been oxidised, solution B remained quite clear, no trace of sulphur was observed and analysis proved that oxidation had not taken place at all It is thus clear that the oxidation of the ferrous salt by sulphur dioxide is inhibited when the ratio Fe''' Fe exceeds 14 per cent This peculiarity of the mixed salts has been confirmed by several experiments, though the limiting concentration for inhibition, like the maximum yield of ferric salt on oxidation, seems to depend to some extent on conditions (temperature, etc) and will need further investigation As in the oxidation-reactions, sulphuric acid could not be detected in the solutions thus treated

It may be noted that this is not a case of a balanced reaction determined by the equal velocities of two reversible processes, but is rather one of double inhibition, as it may be termed, both the oxidising and reducing properties of the sulphur dioxide, with respect to ferrous and ferric salts, being in abeyance, the former by reason of the ferric chloride present, the latter because of the high acid-content of the solutions. The matter is still more complicated if the state of inhibition is reached from the pure ferrous salt, for then sulphur is present and its oxidation by ferric chloride is also inhibited by the acid; a similar remark would also apply to any hydrogen sulphide which might be formed in the reaction.

General Remarks on the Behaviour of Sulphur Dioxide as an Oxidising Agent.

The outstanding characteristics of sulphur dioxide as an oxidising agent, in the above examples, are the slowness with which it reacts and the dependence of the reaction upon the presence of a considerable amount of hydrochloric acid. Such features are met with in other cases, notably in the reaction with the mercaptans; here, the similarity in behaviour of sulphur dioxide and hydrochloric acid to thionyl chloride is striking, as is illustrated by the following, quantitative reactions:—

$SOCl_2 + 4\,R.SH = R_2S_2 + R_2S_3 + H_2O + 2\,HCl$.[17]

$SO_2(+HCl) + 4\,R.SH = R_2S_2 + R_2S_3 + 2\,H_2O\ (+HCl)$.[18]

These facts lead one to think that thionyl chloride may be formed by the reaction of sulphur dioxide and hydrochloric acid and, thus, that the oxidising properties of sulphur dioxide are exercised through the medium of thionyl chloride, initially produced. On this view, the usual reaction of decomposition of thionyl chloride by water would be reversible:

$$SOCl_2 + H_2O = SO_2 + 2\,HCl.$$

Though this reversibility has not been proved, there are analogical, and other, grounds for regarding it as possible.

[17] R. Holmberg. *Annalen*, 1907, ccclix., 81.

[18] J. A. Smythe and A. Forster. *Soc.*, 1910, xcvii., 1195.

Thus the sulphoxides and the haloid acids yield by reversible reaction the dihalides of the corresponding organic sulphides and water e g

$$CH_3 SO_3 C_6H_4CH_3 + 2 HBr \rightleftarrows CH_3 SBr_2 C_6H_4CH_3 + H_2O \quad {}^{19}$$

$$(CH_2 Ph)_2 SO + 2 HBr \rightleftarrows (CH_2 Ph)_2 SBr_2 + H_2O \quad {}^{20}$$

The only evidence of a positive nature we are acquainted with is the proof that when thionyl chloride reacts with mercaptans at low temperatures (0° to $-70°C$) hydrochloric acid and sulphur dioxide are evolved and water is found among the residual products[21] Now as the sulphur dioxide and part of the hydrochloric acid are formed by secondary action of thionyl chloride on water initially produced in the reaction (see first and third equations above) one may infer that all four compounds are capable of coexistence under the conditions of the experiment

The suggestion that sulphur dioxide and hydrochloric acid may react to form thionyl chloride and that the oxidising properties of the sulphur dioxide in the cases dealt with above are directly due to the presence of this compound would account for the favourable influence of the acid on the course of the oxidation for by the law of mass action, the equilibrium would be more affected by increase in the concentration of the hydrochloric acid than by that of any other constituent From the extreme slowness of reaction it would appear that the concentration of the thionyl chloride can only be very small under the experimental conditions It is hoped that experiments at present in progress may throw some light on these matters

Finally attention may be drawn to one peculiarity of these oxidising and reducing reactions of sulphur dioxide viz that they are not reversible for in those of the former class the sulphur dioxide is reduced to sulphur or hydrogen

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[19] Th Zincke and W Frohneberg *Ber* 1910 xliii 837

[20] E Fromm and G Raiziss *Annalen* 1910, ccclxxiv 90 F Fromm *Annalen* 1913 cccxcvi 75

[21] H S Tasker and H O Jones *Soc* 1909 xcvi, 1910

sulphide, while in those of the latter class, oxidation to sulphuric acid takes place, and the conversion of these sulphur compounds into one another is not realisable under the conditions of experiment. Hence it happens that, in some cases, both oxidising and reducing reactions are exothermal, as in the well known example with iodine and hydriodic acid:

$$SO_2\,aq + 2\,H_2O + I_2 = 2\,HI\,aq + H_2SO_4\,aq + 36{\cdot}7 \text{ cals.}$$
$$SO_2 + 6\,HI = 3\,I_2 + SH_2 + 2\,H_2O + 111{\cdot}1 \text{ cals.}$$

The exothermal character of these reactions has been adduced to 'explain' them in accordance with the principle of maximum work.[22] The calculations for the reactions of the ferrous and ferric salts with sulphur dioxide show both of these to be exothermal too, but, whereas the reduction of mercuric chloride with sulphur dioxide is an exothermal process, the oxidation of mercurous chloride by the same agency is a strongly endothermal one, 25 6 calories being absorbed.

It may be urged that the calculations are based on thermal values determined for dilute, aqueous solutions and that the presence of the acid might therefore be a disturbing factor, but it seems unlikely that the deficit would be made good, even if allowance could be made for this.

Hence, it is questionable whether Berthelot's principle gives any insight into these reactions, and whether, in their causal interpretation, we can advance much beyond the position taken up by Bunsen. They take place, to quote the words of Bunsen when referring to the reactions with iodine and hydriodic acid, "weil der Wert der Grössen, welche ich Verwandschaftscoefficienten genannt habe, je nach den Umständen ein veränderlicher ist."[23]

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[22] See, for example, Hans Jahn, "Die Grundsätze der Thermochimie," 3rd Ed., 1892, p. 137.

[23] *Annalen*, 1853, lxxxvi., 261.

THE RATE OF LIBERATION OF HYDROCYANIC ACID FROM COMMERCIAL KINDS OF LINSEED.

By S. H. Collins, M.Sc., and H. Blair.

When linseed is crushed and moistened, hydrocyanic acid is slowly formed by the action of an enzyme on a cyanogenetic glucoside. The amount of hydrocyanic acid and the rate at which it is given off, both depend on a variety of conditions Both amount and rate have a bearing on the safety of linseed as a cattle food *

The object of the present communication is to show some of the variations in the amount and rate of evolution of hydrocyanic acid due to variations in the place of origin of the seed. Under the Essex County Council there have been carried out several experiments to test the practical value of linseeds of different origin. Owing to the kindness of Mr. E. M. Taylor, the chemist to the East Anglian Institute of Agriculture, we have been able to determine the amounts of hydrocyanic acid in many of the seeds grown under experimental conditions. These linseeds yielded in Essex from 10 to 18 cwt. seed per acre, and 10 to 34 cwt. straw per acre. The seeds contained from 30 to 39 per cent. oil and 20 to 23 per cent. albuminoids.

The accompanying table gives the names of the districts from which the seed originated. In all cases the seed was sown and reaped in Essex, and the seeds actually tested were the first year's home-grown seed from crops grown with foreign seed. The column showing the total amount of hydrocyanic acid gives the amounts liberated under the conditions described in vol. iv., p. 99, during a period of 5 hours at 45° C.

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* See "The Rate of Evolution of Hydrocyanic Acid from Linseed under Digestive Conditions," *Univ. Dur. Phil. Soc.*, vol. iv. p. 99, and "The Rate of Liberation of Hydrocyanic Acid from Linseed," *Analyst*, 1914, p. 70.

The column showing the rate of liberation gives the number of minutes necessary to liberate one half of the total amount of hydrocyanic acid The figures in this column vary inversely with the enzymic activity The enzymic activity may vary with the amount of enzyme present, but other causes will also act

The criterion of safety for cattle feeding will be—a small figure in the first column and a large figure in the second column

TABLE I FIRST ENGLISH CROP FROM FOREIGN SEED

	Origin of Seed	Hydrocyanic Acid. Parts per 1 000	Minutes to evolve one half total amount
2	Memel, North Russia	088	70
10	Reval, „	197	60
15	Libau „	193	28
19	Windau	175	35
5	Steppe, Russia	160	35
1	Mancupel, South Russia	170	55
4	Theodosia, „	123	75
13	Berdiansk „	106	90
14	Nicholaieff, „	105	90
16	Eaupatoria, „	173	35
20	Ghenitchesk, „	175	35
1	Koenigsberg, Germany	213	55
7	Kustendji Roumania	183	45
18	Braila, „	115	55
3	Turkey	108	75
9	Morocco	112	60
6	Bombay	210	35
8	Calcutta	233	40
12	Japan	270	55
	Average	164	56

A study of Table 1 shows that there is great variation in both amount and rate The seeds of Oriental origin, Calcutta, Bombay and Japan are all high in total hydrocyanic acid and rich in enzymic activity On the other hand, the seed derived from Morocco is low both in hydrocyanic acid and low in enzymic activity These results are exactly in agreement with those obtained in former years by testing seed direct from foreign countries, and may therefore be regarded as permanent characteristics of the districts concerned

Table 2 gives the results obtained from seed grown in foreign places as stated, together with some samples of seed grown in the College garden which had been grown there for many years but had originally come from India. These results are still further compared in Table 3 which shows the results of growing foreign seed in England. It will be seen that the general result of growing linseed in England is to reduce the total amount of hydrocyanic acid and to increase the activity of the enzyme. These results are in conformity with some previous results obtained by growing linseed in pots in the greenhouse, some being grown with the minimum and some with the maximum amount of water that was compatible with a fair crop. In these cases the linseed grown with the minimum amount of water con-

TABLE II. LINSEED FROM VARIOUS SOURCES

Locality	Hydrocyanic Acid Parts per 1 000	Minutes to evolve one half total amount
College garden, minimum	120	45
,, maximum	170	90
Irish	275	85
Riga	225	65
Morocco	151	55
Bombay minimum	260	45
, maximum	300	62
Calcutta minimum	250	46
,, maximum	380	50
Plate	175	70
Average	231	61

TABLE III.—EFFECT OF HOME GROWTH ON FOREIGN LINSEED

Locality	Hydrocyanic Acid Parts per 1 000	Minutes to evolve one half total amount
Bombay, foreign grown	280	54
,, once home grown	210	35
Calcutta, foreign grown	315	48
once home grown	233	40
Morocco foreign grown	151	55
,, once home grown	112	60
Riga, foreign grown	225	65
,, twice home grown	163	35

tained more hydrocyanic acid and showed a slower rate of evolution than did the linseed grown with the maximum amount of water. Similarly, linseed grown in the garden in the dry summer of 1911 showed more hydrocyanic acid a slower rate than did the linseed grown in the wet summer of 1910.

By collecting all the information under this head and arranging as far as possible a contrast between those linseed grown under conditions of drought and heat with those grown under conditions of damp and cold, the following results are obtained.

The average result of changing seed from dry and hot conditions to damp and cool conditions is to depress the hydrocyanic acid evolved by 20 ± 3 per cent. and to reduce the half time rate by 24 ± 5 per cent.

There is no doubt that these commercial varieties of linseed are mixed and genetically impure. I am, however, in hope that I may be able to obtain some genetically pure seed and continue these investigations. Meanwhile it would seem that English grown seed is safer than foreign grown seed for the purposes of cattle food, but that before further experiments on the growth of linseed are carried out in England, it would be well to see that the seed selected for such experiment was of a variety likely to give a linseed low both in cyanogenetic glucosides and enzymes. There is not much correlation between yield of seed per acre and cyanogen content, but on the whole there is a tendency for the seeds having an origin in temperate climates to give the best yield per acre and to contain the least proportions of cyanogenetic glucosides.

ON THE SKELETAL STRUCTURE OF *BALÆNOPTERA ROSTRATA*, FABRICIUS.

By THOS BENTHAM, B Sc

[Read May 28th, 1914.]

In August, 1913, a whale of the species Balænoptera rostrata, Fabricius was washed ashore near the old rifle-range at St. Mary's Island. It had been seen floating three weeks before off Coquet Island by the crew of the "Evadne." It was then in an advanced state of decomposition. I never had an opportunity of seeing the animal in the flesh, and this would probably have served no useful purpose, as the flippers which were obtained in the flesh were uniformly yellowish-grey in colour due to decomposition, and neither showed any trace of the white-band characteristic of this species. A service bullet was found in the left flipper between the third and fourth phalanges. The right mandibular ramus was missing,[1] as also the two tympanic bones, the fourth right rib, the pelvic bones and the baleen. The last had probably been removed at some whaling-factory at an earlier date and the body had then been cut adrift as commercially useless. The animal was purchased by a fish-salesman at Blyth and the flesh and blubber were utilised as manure. The skeleton was obtained by the Zoological Department of Armstrong College and was found to be of an individual about 26 feet in length. The sex of this animal was not ascertained, but it was very probably a female.

Previous records of this whale have been very few on the Northumberland coast.

In Mennell's paper on the Mammalia of Northumberland but two records are cited.

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[1] There is a probability that this ramus will be received. We have [illegible] d locally

Plate I

SKELETON OF *Balænoptera rostrata*. FABR.

Shewing form of sternum and the attachment of the first pair of ribs.
The stand below is roughly 2½ feet in height.

One taken on the Dogger Bank 17 feet long and recorded by Hunter.

One, a drawing of which is given with the characteristic white patch on the flipper, thrown up near Craster 6th February 1858, 16 feet in length.

One described by Meek, Report of the Northumberland Sea Fisheries Committee 1902.

In my opinion there is a doubt about this specimen being Balaenoptera rostrata. It approximates more to a young species of B. borealis except in the coronoid process of the mandibular ramus which is quite three inches in height, a characteristic of B. rostrata. There are however a number of caudal vertebrae preserved and these all are pierced by foramina through their transverse processes and their centra are much shorter antero-posteriorly than those of the specimen under consideration. They were much more cuboid with their angles rounded off and the bone though it had been much exposed was not of the characteristic whiteness shown in specimens of B. rostrata. The mandibular ramus was also much narrower from side to side than in the present specimen, although the coronoid process as above mentioned was typical of the species.

From what I can ascertain all the characters of this specimen agree with those of a small specimen of B. borealis except in the character of the lower jaws. Many systematists would place this whale as an entirely new species or at any rate as a sub-species.

Lastly, in November an anterior lumbo-sacral vertebra was brought in to the Dove Marine Laboratory from one of the trawlers. It was undoubtedly the vertebra of a small whale of this species and was unmistakeable owing to the whiteness of the bone. In comparison with the specimen in our possession this animal would have measured about 16 feet in length.

In the course of articulation this whale appeared to present certain peculiarities of structure not noticed by previous authors. It was therefore considered worth while to write a short paper showing the skeletal structure as completely as possible, to point out details in which this whale differed from those described by other authors. Turner's description is that of a whale very much the same size as this specimen, as also that of Lilljeborg. Other authors have described specimens which were decidedly immature. Turner does not describe his specimen in very great detail nor does Lilljeborg. Carte and Macalister give a very full description of an immature specimen.

AXIAL SKELETON

The Skull (see Plate II, Figs. A and B).—The skull was roughly triangular in shape, the triangle being isosceles with basal angles of 75°.

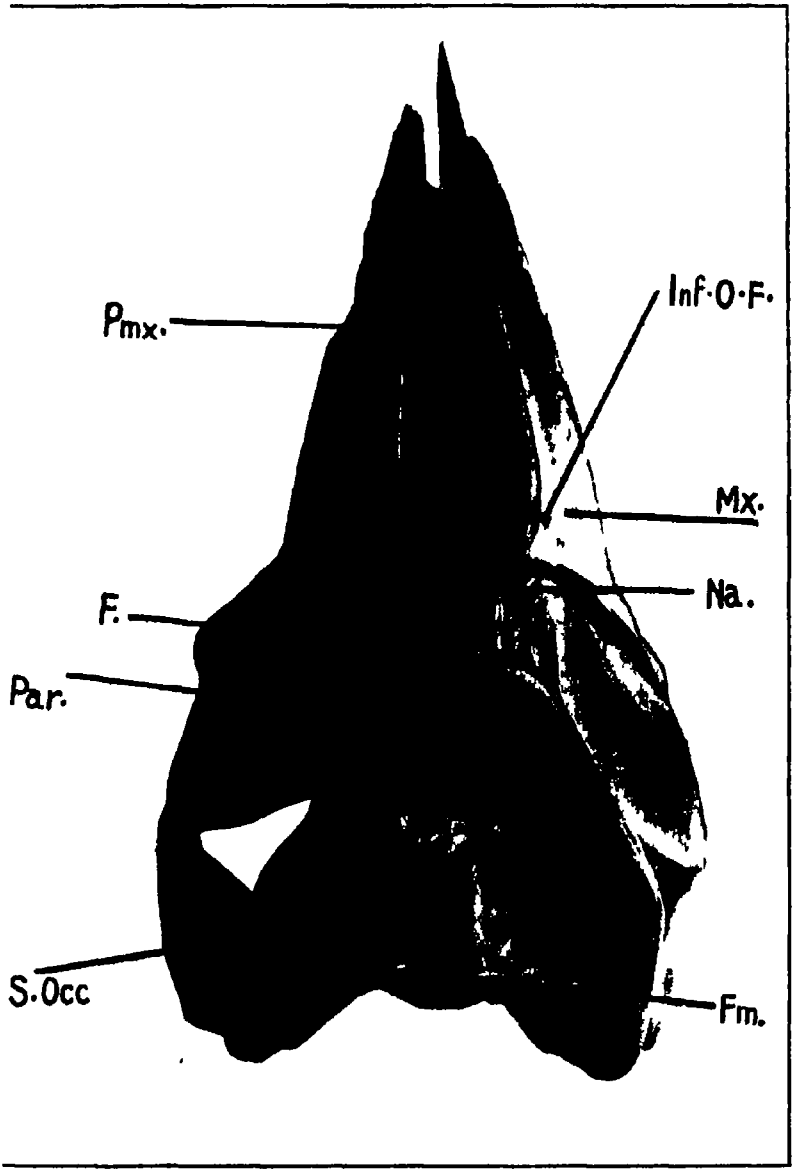
The roof of the calvarium was heart shaped with a convex base and consisted wholly of the occipital elements, constituting the largest bone in the skull.

The apex of the heart-shaped supra-occipital articulated with the frontal which was intercepted between the former and the maxillæ as a mere compressed ridge. The surface of the occipital was raised into a rounded eminence—the occipital crest—above the opening of the foramen magnum and there were three large shallow depressions arranged—one near the apex of the occipital—the other two one on each side and in front of the occipital condyles.

The exoccipitals were closely fused with the supra-occipital and remains of sutures were only found as two notches lying about three inches to the side and below the condyles on the limiting edge of these two bones.

The occipital condyles were two kidney-shaped convex elements closely fused along their lower aspect and enclosing between themselves and the supra-occipital an irregularly oval foramen magnum which pointed somewhat upwards, its shortest width being in a line with the long axis of the skull. It measured 3 inches by 2¾ inches.

The basi-occipital extended forward along the base of the skull and articulated with the basi-sphenoid, its lower

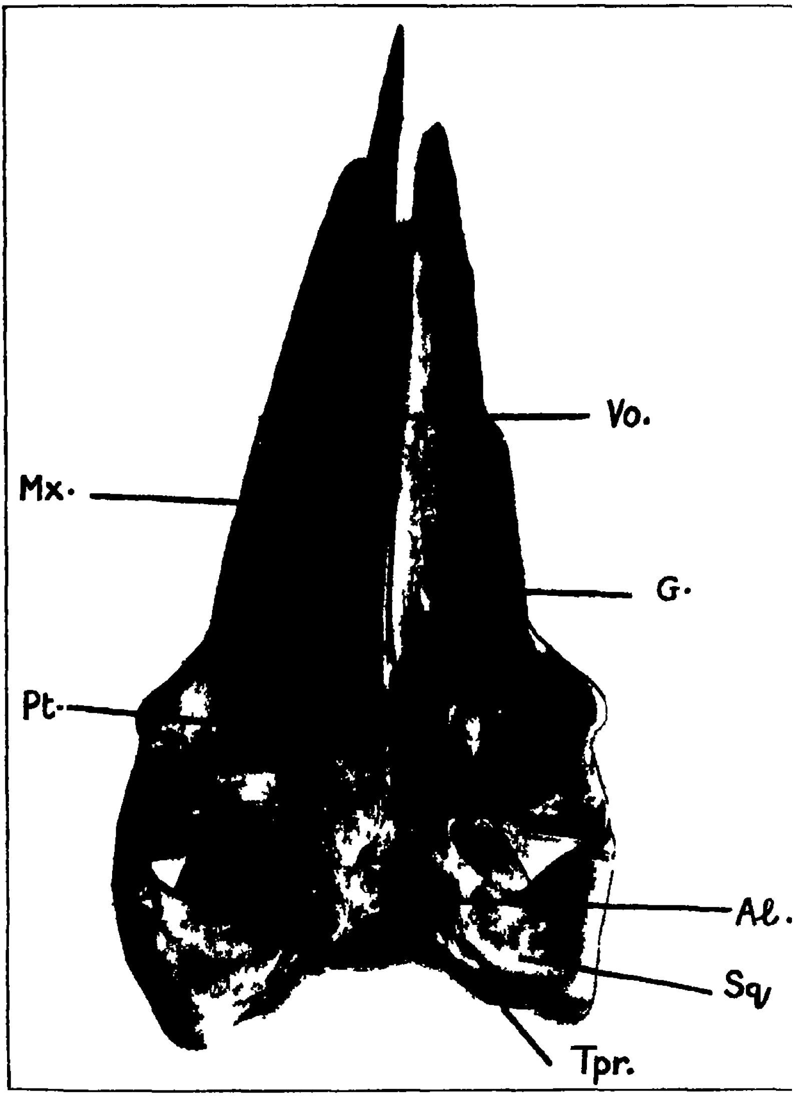


SKULL OF *Balænoptera rostrata*, FABRICIUS $\frac{1}{18}$ SCALE

A FROM THE DORSAL SURFACE

Pmx Premaxilla Inf O F Infra orbital foramen Mx Maxilla Na Nasal F Frontal
Par Parietal S Occ Supra-occipital Fm Foramen magnum.

Plate II



sn pl tes Pt Pterygo

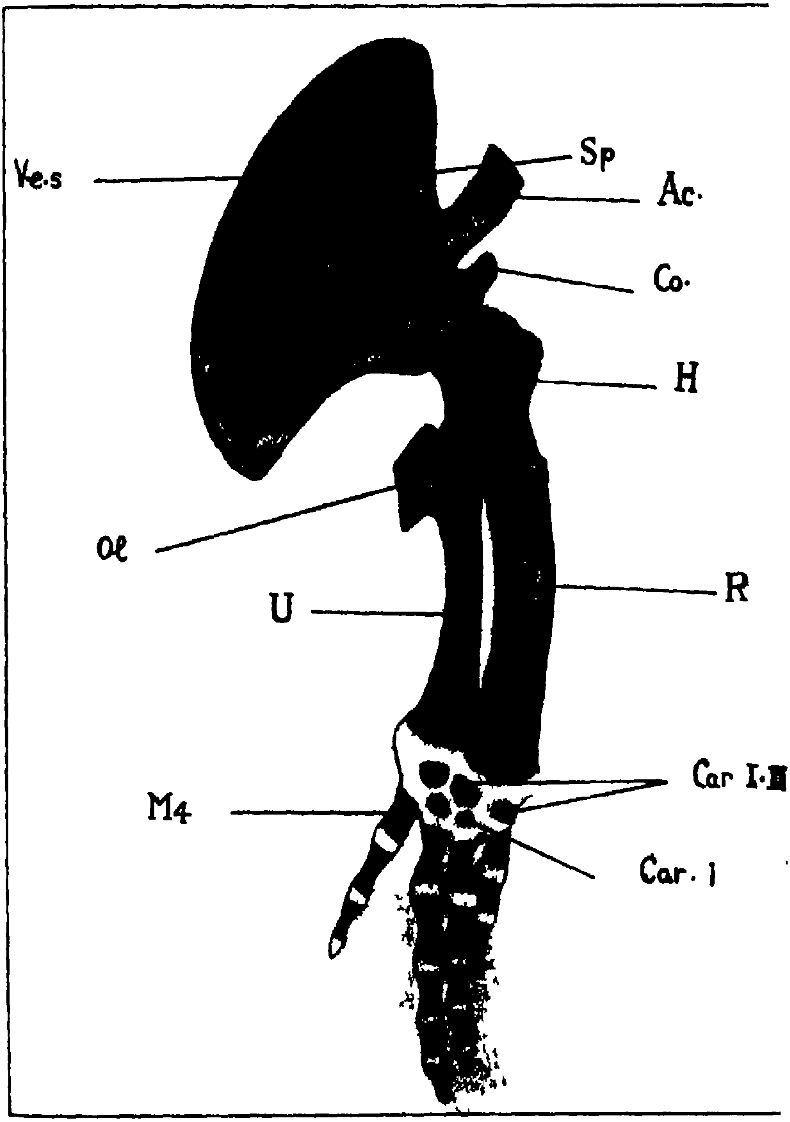
portion being extended outwards and forwards into two wing-like processes which lay close to the lower border of the tympanics. Its upper pointed portion was interposed between the two lower parts of the pterygoids which overlapped it. There was a well-marked sulcus extending across these wing-like processes and also separating them into two elements at their outer ends.

The Squamosal was large, long, roughly triangular on its under surface with a large shallow concavity for the reception of the articular processes of the mandibular ramus. Its upper angle was truncated and articulated some distance from the end by a cartilaginous pad with the descending process of the frontal. Its inner angle was produced into a hammer-shaped process—the squamous plate, the upper end of which fitted into a corresponding notch in the parietal, the lower end being attached to the basi-sphenoid. Interiorly this process was pierced by the opening of the Eustachian tube. The lower external angle of this bone formed the posterior lateral limit of the skull. The interior end articulated by thin intervening cartilage with the lower border of the frontal, while the posterior was acutely pointed and formed the posterior limit of the skull on either side. Two grooves were present at the junction of this bone with the auditory portion of the skull. They ran backwards and outwards, the upper groove carrying an elongated tenon-like process of the periotic. On its upper surface the squamosal articulated with the posterior edge of the supra-occipital.

The Frontal was large, solid and occupied practically the whole of the floor of the temporal fossa. This bone was much compressed behind the maxillæ by two wedges formed by the parietals one on each side, and by the anterior portion of the supra-occipital. It was continued up between the lower processes of the maxillæ as a narrow wedge which articulated with the nasals. Towards the sides it enclosed the descending processes of the maxillæ and appeared only as a narrow strip half-way along the distance to its anterior point. As far as this point it was overlapped by the parietal. In the temporal region of the skull it was concave in contour but here it was overlapped by the parietal so that this bone completely fitted into it The main portion of the bone formed the wall of the temporal fossa and was quadrangular, flat on its upper surface and exceedingly thick and massive At its lower outer angle it articulated with the spuamosal by intervening cartilage Its inner anterior angle was reflected backwards to form a flat triangular plate for the articulation of the inner lower border of the maxilla The anterior border of this bone was narrow and ridge-like and fitted into the upper grooves formed by the lower surface of the maxillæ The inner edge was limited by the overlapping parietal and the outer edge was broad and expanded at its posterior corner for articulation with the squamosal at its upper for articulation with the zygomatic process of the maxilla The internal exposed surface was somewhat triangular and was furnished with a large spout shaped groove which was terminated in the foramen for the optic nerve This foramen was formed by the apposition of two ridges enclosing a groove, the lower ridge overlapping the upper so that the meatus had the appearance of a piece of paper coiled to form a conical bag

The Parietal was a large concave bone occupying the most of the roof of the temporal fossa Behind the apex of the supra occipital it was compressed as two triangular wedges of bone which did not meet in the middle line Internally and superiorly it almost completely overlapped the frontal and articulated behind with the anterior edge of the alisphenoid on the one hand and the squamous plate on the other Behind the free edge of the squamous plate a triangular slip of bone ran down between the former and the edge of the occipital

The Palatine was quadrilateral in shape, its inner edge straight and but slightly concave and lying close to the median ridge of the vomer Its upper edge was incurved towards the median line but convex and continued into the curve of the outer edge Its concave upper surface presented a deep groove which fitted over the lower median end of the



RIGHT FORE ARM OF *Balaenoptera rostrata* FABR. ⅛ SCALE

Ac Acromion Car I II 1st and 2nd carpalia of proximal row Car 1 1st carpal of distal row
Co Coracoid H Humerus M4 4th metacarpal Ol Olecranon R Radius
Sp. Spine U Ulna V e s Vertebral edge of scapula.

maxilla. Its lower lateral edge was produced forward into a hook-shaped process and was itself rounded in contour.

On the lower anterior inner surface a raised irregular area was present which consisted of a number of compressed laminæ which interdigitated with similar laminæ on the under surface of the alisphenoid.

From the outer upper corner a well-marked ridge extended diagonally across the bone to the region where the two palatines became sharply separated enclosing a triangular space below.

The Maxillæ were large cultriform bones and constituted the major portion of the anterior part of the skull. They presented four surfaces and were in shape elongated, triangular, hollow, pyramids, one, the lower side of which had been removed The nasal surfaces of these bones were both closely approximated to the vomer and almost met in the middle line, exposing but a small strip of the latter on the under surface. Their upper posterior edges were turned over so as to overlap the edges of the vomer. On the upper edge of the angle formed by the nasal and facial surfaces there was a distinct groove extending half way along this bone from behind forwards. This groove served as a slot for the reception of a ridge on the upper inner surface of the premaxilla.

The facial surface was in shape a scalene triangle, its inner edge being the longest and its outer the next longest side. It was slightly convex in contour, its shorter posterior edge being produced into a long somewhat hook-shaped zygomatic spine, extending slightly beyond the outer margin of the frontals. Its longest or inner edge was continued down as far as the frontals and formed one side of an oblong nasal process lying between the nasals, frontals, and supra-occipital. On this surface the bone was pierced by five groove-like foramina on the left side and seven on the right. The infraorbital foramen was about three-quarters of an inch in diameter and was situated about five inches in front of the commencement of the nasal process. The inferior palatine surface was much hollowed out for the reception of the baleen and constituted the greater portion of the palate It was excavated by a large number of grooves ending in foramina for the blood vessels to the baleen plates The temporo frontal surface or base of the pyramid was triangular in outline and crenated at its hinder edge into a number of irregularly shaped lobes which decreased in size towards the median line This surface was closely approximated to the inferior surface of the frontal but overlapped this bone with its upper edge

The Premaxillæ projected slightly beyond the maxillæ Their anterior halves were triangular in section the upper side being markedly convex their inner and outer concave In consequence of this three sharp inner outer inferior edges were shewn the outer of which fitted into the grooves of the maxilla the inferior with a space between the vomer and the maxilla Anteriorly this bone tapered to a somewhat blunt point and posteriorly became more flattened and twisted on its own axis so that the inner ridge which had formerly looked directly inwards now looked inwards and downwards becoming a mere ridge which overlapped the lateral wall of the vomer

The Vomer was a long spout shaped bone tapering towards its anterior extremity Superiorly it was deeply channelled this channel being widest at its posterior third the walls being higher and most widely divaricated in this region Inferiorly and at its posterior end it was ridged but became less so anteriorly Below it was exposed between the maxillæ forming part of the palate Posteriorly the channel became narrow and accommodated the ethmoid apparatus The basal end of this bone was interposed between the frontal anteriorly and the upper pterygoid portion of the alisphenoid laterally

The Nasals were small solid beak shaped bones and were closely approximated in their middle line the apposing surfaces being uniformly flat with the exception of a small process projecting from their anterior lower edges Posterior ly they tapered to a somewhat truncated point with a V-shaped notch on it. The anterior surfaces were smooth, curved forward, and convex, the posterior being broken up into a number of deep laminæ which fitted into the grooves formed by the laminæ of the frontal bone. The inferior surfaces were quadrangular in outline and formed part of the roof of the nasal chamber. Their outer surfaces lay alongside the flattened ends of the premaxillæ.

The wing of the sphenoid was an irregular bone partly enclosing the tympanic. Its upper end articulated with the inner corner of the frontal at the point where the optic groove began to be completely closed into a foramen. The lower end sent back a square plate of bone almost at right angles to the main body. This articulated squarely with the end of the squamous plate and with the inner corner of the frontal. The upper surface was deeply laminated for the reception of the palatine. The inner surface of this bone was continued into a pterygoid process, a square bone which articulated inside with the lower end of the vomer by a number of hook-like projections forming a rough suture. On the outer surface the bone was free forming a somewhat convex plate for the reception of the inner side of the tympanic.

The two pterygoid processes did not, as Carte and Macalister state, unite below to form a hamular process. The body of sphenoid is not described, because it was not sufficiently exposed.

The Ethmoid Bones consisted at their anterior end of a flat plate which was attached to the frontal. Along its middle line were two elevated ridges—the moieties of each bone—which enclosed a deep channel leading down into the nasal cavity. At the base of each ridge was a deep foramen for the nasal nerve, formed by an upcurved process of the square plate which was attached to the vomer.

The Mandibular Ramus extended some few inches beyond the ends of the premaxillæ. On its inner surface it was flattened and slightly concave and on its outer was sharply rounded On the latter surface it was grooved by a number of channelled foramina The coronoid process was much elevated and was four inches in length—a characteristic of the species

The Hyoid consisted of a body closely fused to which were two posterior cornua wider at their fusion to the body than at their ends The anterior notch was very deep, measuring one and a half inches Its two sides, formed by styliform processes were unequal the left being distinctly longer than the right Carte and Macalister figure this depression as quite shallow The stylo hyals were articulated by intervening cartilage just behind the two processes forming the notch and in this region the bone was deeply pitted for the reception of the connecting cartilage They were solid, slightly curved bones having on their anterior distal edge a distinct flattened ridge which made this part of the element broader than the proximal end Both ends of this bone were epiphysial in character

The first three cervical vertebræ were completely fused by their centra, the third being also fused by its transverse processes Although fused the epiphysial plates were all quite distinct There is only one case of the fusion of the first three cervicals and this has been quoted by Flower The commonest occurrence is to have all three vertebræ free, cf Carte and Macalister and Lilljeborg but Turner quotes the fusion of the axis and the third

The Atlas was a quadrangular bony ring measuring in its greatest length one foot one and a half inches and in its greatest breadth eight inches It consisted in front of two reniform cups meeting at their lower extremities Those cups accommodated the occipital condyles and were divided at the top and bottom by two triangular interstitial ossifications The transverse processes were solid and formed with the neural arch three elements in the shape of equilateral triangles, separated by the reniform fossa The neural arch was pyriform internally, the broader conformation being at the top of the arch below the spine, which last was low and directed slightly backwards. The opening of the channel for the sub-occipital artery was completely ringed with bone, and pierced the base of the neural arch in a direction parallel to the line of the transverse processes, which were but slightly directed backwards.

The Axis was the largest cervical vertebra having a breadth of one foot four inches and a total height of nine inches. The odontoid process did not completely fill the lower narrower portion of the neural canal of the atlas and in section it was roughly triangular.

The pleurapophyses and diapophyses were fused and sharply directed backwards, enclosing large oval foramina measuring two and a half by one and a half inches in breadth. The neural arches sloped distinctly forward and shewed anteriorly and about half way down their length two roughened tuberosities one on each side and situated asymmetrically, the left being the lower of the two. Owing to fusion further details of the structure of the axis could not be made out, but the zygapophyses on the outside surface were pointed and incompletely fused to the third cervical.

The Third Cervical was a more slender and compressed bone than the axis, and measured in length one foot and in breadth eight inches. The diapophyses and parapophyses were separate at their distal ends by a space of one and a half inches. They partly enclosed a much larger space than those of the axis. The zygapophyses were one inch in length and oval in outline. Contrary to the statement of Turner the neural canal was complete above and there was a small tuberosity present on the right neural arch.

The Fourth Cervical was the smallest of the series and measured ten inches by six inches. It consisted chiefly of centrum; and the neural canal was incomplete above, the arches being distant from one another by a space of one eighth of an inch, and in consequence of this there was a complete absence of a neural spine. The diapophyses and pleurapophyses were separated by the distance of three inches.

The Fifth Cervical was larger and measured one foot one inch along its diapophyses. The neural arch was complete but the spine was reduced to a mere ridge.

The Sixth and Seventh Cervicals were larger than the fifth, the seventh in its turn being larger than the sixth. In the seventh the neural spine was longest and sharply pointed. On the seventh cervical there was scarcely a trace of parapophyses, these being represented by a minute nodule of bone about a quarter of an inch long projecting from the left hand side of the lower part of the centrum. Other authors state that this rudiment is always present on the right hand side of the vertebræ. The diapophyses of the seventh were long and more sharply curved downwards and forwards than those of the fourth and sixth vertebræ, and its posterior epiphysis was loose and not fused to the centrum. Across its diapophyses this vertebra measured one foot one inch, and these processes were longer than the transverse processes of the first costal vertebra.

The first five cervicals had their diapophyses directed backwards but not so sharply as that of the axis. On the sixth cervical the diapophyses were straight out. The diapophyses of the seventh cervical were longest and were strongly directed forward so that they came in contact with the ends of the parapophyses of the fifth. The third and fourth cervicals had their parapophyses directed forward, while those of the fifth and sixth were directed but slightly forward.

Costal Vertebræ. These vertebræ were eleven in number. The ends of all their transverse processes had facets for the articulation of the ribs. On the first to sixth the transverse processes were directed forwards, the first having a transverse process with two heads. The processes of the seventh were directed straight out and those of the eighth to the eleventh slightly backwards.

The Lumbo Sacrals.—In the first lumbo sacral vertebra the transverse processes were straight out and the widest diameter of the neural canal in this and the succeeding was in a plane parallel to the transverse processes, which last pointed slightly forward in the second to the tenth. The processes of the eleventh and twelfth were again perpendicular to the axis. If twelve lumbo-sacrals be counted the conditions found in this skeleton do not agree with Lilljeborg's description of his whale. In this specimen the ninth caudal vertebra is the last with a neural spine. Carte and Macalister state that the seventh caudal vertebra is the last with a neural spine, while Lilljeborg says the eighth. Furthermore the latter author states that the first caudal is not ridged below. If twelve lumbo-sacrals be counted the first caudal is definitely ridged below, and to bring the description in line with that of Lilljeborg thirteen lumbo-sacrals must be counted and it must be taken for granted that the last caudal is missing. This may have been overlooked as being cartilaginous when the skeleton was prepared. Turner states that in his specimen there were thirteen lumbo-sacrals. On the nineteenth of the lumbo-sacro-caudal series the lateral processes were rudimentary, and there were slight indications of these processes on the twentieth.

Carte and Macalister however state that the lateral processes are entirely suppressed at the nineteenth which is their seventh caudal. They further mention the fact that "the articular processes or zygapophyses of all the lumbo-sacrals vertebræ articulated with each other while those of the caudals were free."

In the present specimen the zygapophyses articulated with the neural spine in front on the first fourteen lumbo-sacro-caudal vertebræ.

The foramina (mentioned by the last two authors) on the dorsal aspect of the sixteenth vertebra were absent at the origin of the transverse processes as also on the seventeenth. Foramina were present on the eighteenth and nineteenth lumbo-sacrocaudals, the nineteenth being the last with a definite transverse process. The apertures in the succeeding vertebræ perforated the sides of the dorsal aspect of the centra.

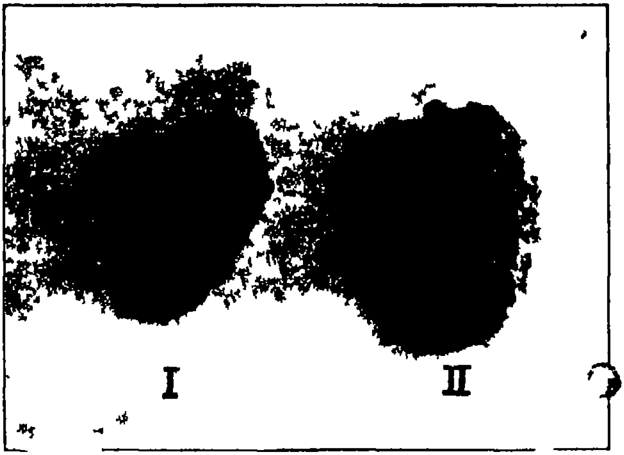
Caudal Vertebræ. The ten terminal caudals had no neural canals, these being merely represented by two ridges, the remnants of the neural arches. The centra were uniformly cylindrical and in shape resembled the centra of the previous vertebræ. They could not be described as bony cubes (cf. Carte and Macalister).

The Chevron Bones. - These were seven in number, the second being the largest of the series They were V-shaped bones, the upper end of the V articulating with corresponding ridges on the caudals They each articulated between two successive bones, the first being between the second and third caudals. They were in the following order of size, viz., 2, 3, 4, 5, 6, 7, 1. Number four was stouter and broader than number three, which was only superior in length.

The Ribs. - (Plate I.). - The first pair were smallest in length, but widest at their distal extremities, having a distinct process running backwards parallel to the inner edge of the rib, and commencing about four inches from that extremity. They did not articulate directly behind the lateral processes of the cross-shaped sternum but the upper angles of their extremities articulated about the middle, and gradually narrowed towards their distal and proximal extremities. They were all much flattened on their inner, but were more or less convex on their outer aspects.

The fourth and fifth were the longest, and all had a distinct capitulum and neck except the ninth, tenth, and eleventh.

The Sternum.—(Plate I.).— This bone was represented by a single element corresponding to the præsternum of other mammals. In shape it was like a latin cross, but the angles between the four moieties composing the cross were not sharply defined were denoted by simple curves. The posteriorly and downward directed process was longest and represented the handle of the cross.



FIGS. 1 & 2. ULNAR AND RADIAL EPIPHYSES FROM LEFT ARM OF *Balaenoptera rostrata*. [illegible] SCALE.

I. Ulnar epiphysis. II. Radial epiphysis.

APPENDICULAR SKELETON.

The Scapula. –The scapula was a large falcate bone broader than long with a comparatively smooth outer surface, its inner surface being marked by three short and flat ridges, which radiated from the neck just behind the coracoid process. Its vertebral edge was a slight curve and smooth about its middle portion, becoming more broken up and tubercular towards the limit of the curve. Its anterior edge was flattened and faced the inner surface of the acromion. Two ridges were thus formed on each side of this flattened edge. The outer ridge denoted the remains of the scapular spine and was continuous with the posterior edge of the acromion. The inner, which limited the anterior edge of the scapula, was slightly incurved and was continued downwards and along the upper edge of the coracoid The lower edge was concave in contour, thicker towards the neck and sharply tapering towards the posterior region where it formed with the vertebral edge the falcate appearance above described. The acromion process was sharply turned upwards and formed an acute angle with the spine of the scapula. It was somewhat incurved and thus concave on its inner aspect. The coracoid was a smaller process and was turned in towards the vertebræ forming an obtuse angle with the transverse axis of the scapula. Its lower edge arose directly from the anterior edge of the glenoid cavity.

The glenoid cavity was a shallow oval concavity, pitted, tuberculated, and lined with thick epiphysial encrusting cartilage.

The Humerus.—The humerus was a stout, short, club-shaped bone the head of which was smooth, hemispherical, and covered with encrusting cartilage. The head itself was too large for the glenoid cavity of the scapula and consequently did not completely fit into that depression, the major anterior portion being quite free. The external tuberosity was large and well marked. It lay anteriorly to the head and was separated from this by a distinct broad groove which was continued almost round the shaft below the head. It was coarsely pitted and tuberculated over its entire surface and its most conspicuous projections were two in number, a large mastoid prominence extending inwards towards the ribs, and a small downward pointing process lying at its lower anterior border. The shaft was short and laterally compressed so that it was oval in section. The surface was finely grooved on its inner aspect and pitted on its outer. The distal depression for the head of the radius was larger than that for the accommodation of the head of the ulna, its epiphyses being also much thicker and stouter.

The Radius and Ulna.—The radius and ulna were subequal bones approximated only at their proximal and distal extremities, the radius being much stouter and projecting further into the carpal cartilage. The proximal ends of both these bones were furnished with flat plate-like epiphyses, which were more completely fused than those of the distal extremity of the humerus. Anteriorly and at its junction with the humerus the radius projected away from the line of the anterior aspect of the former, so that its articulating portion represented but five-eighths of the proximal surface of the bone. The ulna was a more slender bone, narrower below the olecranon process than at its distal end, where it was in breadth equal to the same end of the ulna. The upper articulation of this bone was partly carried out by the ascending anterior surface of the olecranon, which was broad and turned acutely downwards at its junction with the shaft. The posterior border of the olecranon process was tuberculated and pitted for the reception of a large triangular slip of cartilage.

Ulnar and Radial Epiphyses.—(see Plate IV., Figs. 1 and 2).—At the bases of the ulna and radius in a cup-shaped depression of these bones two epiphyses were found completely embedded in the cartilage. The larger of the two was that accompanying the radius. They were ovoid bones resembling large plum-stones in shape and were deeply pitted and tuberculated over their entire surfaces, and resembled the surfaces of the carpalia which were embedded in the cartilage and not exposed to the surface. In appearance they much resembled similar bones described by Flower in Physeter.

The Carpus.--The bones of the carpus were five in number and corresponded in all probability to the trapezio-scaphoid, the semilunar, and the cuneiform of the proximal row, and with the trapezo-magnum and the unciform of the distal row.

All these bones were, when the cartilage had been removed, cylindrical in shape with the exception of the trapezo-magnum. They were smooth on both their exposed surfaces, but were deeply pitted and tuberculated on the borders surrounded by the cartilage. The measurements across the middle portion of the cylinder were invariably greater than the diameter ot the exposed surfaces with the exception of the case of the trapezo-magnum which was peg-top shaped, its outer surface being smooth, its inner being a smooth truncated point which was just visible on the inner or palmar side of the carpus. Of the five bones the semilunar was imperceptibly the largest and presented an equal palmar and outer aspect. Next in order of size were the cuneiform, the trapezio-scaphoid, the unciform and the trapezo-magnum. All, with the exception of semilunar and trapezo-magnum presented a smaller surface on the outer than on the inner surface of the carpus. The outer and inner surfaces of the trapezio-scaphoid were bevelled round to meet one another at the anterior edge of the carpus, so that the bone fitted the rounded contour of the cartilage at its front edge, and did not project away from it. As in the toothed whales the surrounding cartilage exhibited grooves surrounding each bone, so that it appeared to possess a distinct pentagonal area of cartilage to itself. The greatest amount of cartilage was present between the ulna and radius and the proximal row of carpals, but a tongue-shaped portion of cartilage could be traced upwards and backwards from the top of the insertion of the fourth digit. This was pentagonally grooved as in the main bulk of cartilage surrounding the carpalia, and represented the pisiform bone, which had not yet become ossified.

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The following are measurements of the several elements of the carpus

	Outer Surface Inches		Palmar Surface Inches	
Cuneiform	1¾	1⅝	1½	1⅜
Semilunar	1	1⅝	1⅞	1⅜
Trapezio scaphoid	2	1¼	2	1¼
Unciform	1	1⅛	1⅝	1⅛
Trapezo magnum	1	⅝	⅞	⅝

The digits were in the following order of size—3, 2, 1 4, and the number of phalanges was greatest in digit 3 there being 5

In order	1st digit	3 phalanges
	2nd ,,	4 ,,
	3rd ,,	5 ,
	4th ,,	2 ,,

The longest metacarpal was that belonging to digit 3

The following is a list of measurements in inches comparing the present specimen with that measured by Lilljeborg from the vicinity of Bergen

It may be mentioned that a Swedish foot equals 11 69 inches of English measurement

	Bergen Specimen		St Mary s Island Specimen	
	Feet	Ins.	Feet	Ins
Length of skeleton	23	0	23	8
Length of skull	5	2½	5	8
Width of skull across temporal bones	2	11⅝	2	11
Width of beak at base	1	9	1	9
Width of beak at middle	1	1¼	1	4
Length of beak	3	4¾	3	5½
Length of lower jaw along curve on outer side	5	5⅝	5	6
Circumference of lower jaw at middle	1	0	1	0½
Height of processus coronoideus behind		3		4
Length of centrum of atlas		2		1¾
Width of atlas across transverse processes	1	0	1	1½
Length of its lateral processes		2⅝		3 1/16
Length of centrum of axis		2½		2⅝
Width of axis across lateral processes	1	4¾	1	3⅛
Length of its lateral processes		6		6
Length of centrum of 3rd cervical vertebra		1⅛		1½
Length of centrum of 7th cervical vertebra		1½		1½
Length of centrum of 1st dorsal vertebra		1¾		1⅛

	Bergen Specimen		St Mary s Island Specimen	
	Feet	Ins	Feet	Ins
Width of centrum of 1st dorsal vertebra		5		5½
Length of lateral processes of 1st dorsal vertebra		6		6½
Length of centrum of 11th dorsal vertebra		5		5
Width of centrum of 11th dorsal vertebra in front		5½		6
Length of lateral processes of 11th dorsal vertebra		9½		9½
Length of neural spine of 11th dorsal vertebra		9½		9½
Length of centrum of 1st lumbo sacral vertebra		5		5
Width of centrum of 1st lumbo sacral vertebra in front		5⅝		6
Length of lateral processes of 1st lumbo sacral vertebra		9½		9½
Length of neural spine of 1st lumbo sacral vertebra		9½		11
Length of centrum of 7th lumbo sacral vertebra		5¼		6½
Width of centrum of 7th lumbo sacral vertebra in front		6		6½
Length of lateral processes of 7th lumbo sacral vertebra		8½		10
Length of neural spine of 7th lumbo sacral vertebra		11	1	1½
Length of centrum of 13th lumbo sacral vertebra		7¼		7½
Width of centrum of 13th lumbo sacral vertebra in front		6½		6½
Length of lateral processes of 13th lumbo sacral vertebra		5		6
Length of neural spine of 13th lumbo sacral vertebra		7		9½
Distance between the outer edges of processus obliqui of 13th lumbo sacral vertebra		2¼		2½
Length of centrum of 2nd caudal vertebra		7¼		7½
Width of centrum of 2nd caudal vertebra in front		6¼		—
Length of lateral processes of 2nd caudal vertebra		4½		5⅛
Length of neural spine of 2nd caudal vertebra		5⅝		7½

	Bergen Specimen		St Mary's Island Specimen	
	Feet	Ins	Feet	Ins
Length of centrum of 6th caudal vertebra		6⅛		7
Width of centrum of 6th caudal vertebra in front		6½		6½
Length of centrum of 13th caudal vertebra		1⅝		$2\frac{3}{10}$
Length of sternum	1	2¼	1	4
Width of sternum		9½		8½
Length of scapula	1	2	1	3
Width of scapula	1	11½	2	2¼
Length of acromion		5½		7
Length of coracoid		3½		4
Length of humerus		10		9¼
Width of humerus at middle		4¾		5
Width of humerus at lower end		5⅛		6
Length of radius in a straight line	1	5	1	5¼
Width of radius at the middle		3⅜		3¾
Length of ulna in a straight line without olecranon	1	3¼	1	3¾
Width of ulna at the middle		2⅝		2½
Width of ulna across olecranon		5¼		6

To Professor Meek I am indebted for much kindly criticism and advice The plates are from photographs by the late Miss Jamieson

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Dove Marine Laboratory,
Cullercoats,
Northumberland

August 31st, 1914

BOULDERS COMMITTEE.

REPORT NO. 7. JUNE, 1914.

Reported by Dr. WOOLACOTT and C. T. TRECHMANN, B.Sc.—

Hendon Banks, near Sunderland. Threlkeld "Granite", Volcanic series of Borrowdale. Eycott Hill lava.

Sand pits, Durham. In current bedded sands and leafy clays. Volcanic series of Borrowdale (numerous), Threlkeld "Granite", Greywacke, Red granite, Quartzite; Sandstone; Coal.

Banks of Wear above Durham. Volcanic series of Borrowdale; Thelkeld "Granite"; Sandstone, Whin.

Cleadon sand pit Chert, Brockram.

Marsden quarry. Greywacke.

Man Haven. Volcanic series of Borrowdale.

Reported by Dr. SMYTHE—

Coast near Howick. In Pebble bed resting on boulder clay. Magnesian limestone (with fossils), Whin; Greywacke; Chert, Mica schist, Cheviot porphyrite and andesite; Olivine basalt.

Reported by Dr. SMYTHE and Dr. WOOLACOTT—

Fallowlees Burn, Bardon Mill. Volcanic series of Borrowdale, Granite (Skiddaw?), Thelkeld "Granite", Gabbro (Carrock Fell?).

Kingswood Burn. Same as in last, with Penrith sandstone, agglomerate, spotted schist, and various granites

Reported by C. T. TRECHMANN, B.Sc.—

Many Scandinavian boulders from Durham coast, about a mile north of Castle Eden Dene (a fuller report will appear in a subsequent number of *Proceedings*).

STRIATIONS.

Reported by Dr. SMYTHE—

30 yards south of Saddle rock, Dunstanburgh, on limestone. Two directions, S. and 25° E. of S. (sea level). Embleton quarry, on Whin Sill, 25° E. of S. Ratcheugh quarry, on Whin Sill. Three directions, S., 18°-25° E of S., 5° E. of S. ¼ mile south of St. Oswald's Chapel, 24° E. of S. Height 760 feet.

Reported by Dr. WOOLACOTT and C. T. TRECHMANN, B.Sc.—

Hendon Banks, near Sunderland, on magnesian limestone, 60° E. of S., sea level.

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Vol. IV., Part 6	31 11 4	
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		79 1 6
Balance on deposit Newcastle Savings Bank		3 8 8
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Audited and found correct,

C. M. JESSOP.

October 28th, 1913.

UNIVERSITY OF DURHAM PHILOSOPHICAL SOCIETY

EDITORIAL

The activity of the Philosophical Society has been very much interrupted by the war Many members have joined the Army and Navy and the few who remain have had little time for private work the whole of the Armstrong College buildings were at the outbreak of war taken over for the 1st Northern General Hospital and other colleges have been seriously disorganised so that it is not surprising that instead of two parts of the *Proceedings* each of about sixty pages only this short number has been published Nor does it at present seem possible that next session will see any improvement though the Editorial Committee hope that some means may be found of preventing the complete cessation of the work of the Society

THE ESTIMATION OF SMALL QUANTITIES OF LIME IN PRESENCE OF LARGE QUANTITIES OF MAGNESIA.

By J S F GARD, B Sc

[Read February 11th, 1915]

During last session, in the course of a paper on Dolomites, one of our members remarked on the difficulty of obtaining concordant results in the estimation of lime when associated with large quantities of magnesia, and it occurred to me that it might be useful to put on record the method worked out and used in the laboratory with which I am connected At first considerable difficulty was experienced in the estimation of the lime, duplicates varying from 0·5 to 3 per cent owing to the precipitation of magnesium oxalate, or a double calcium magnesium oxalate which could not be separated by three or four reprecipitations. Only after ignition, re-solution and precipitation could the magnesia be eliminated There appears to be very little literature on the subject. I have found, however, that the oxalate method is very reliable when properly manipulated, and more so than any other methods tried. Among the latter I may mention precipitation of the lime by sodium tungstate; with oxalic acid in the cold (in presence of glycerine); and as sulphate with ammonium bisulphate The tungstate method was the most hopeful of these, occasionally giving good results; it is fairly rapid, but suffers from the great drawback that the calcium tungstate usually sticks so tenaciously to the beakers as to be incapable of removal.

To come to the point, the best results are obtained in the following manner:—The equivalent of 5 grammes of magnesium oxide is dissolved in water, or a slight excess of hydrochloric acid, about 5 grammes of ammonium chloride are added and then ammonia until the solution is just alkaline.

The solution is boiled, and the silica, iron, etc., filtered off; acetic acid is then added in fair excess, and the volume made up to about 450 c.c. with water. The solution is boiled, 50 c.c. of a cold saturated solution of ammonium oxalate poured in, the whole kept at the boiling point for 5 minutes, and afterwards allowed to stand for at least 20 hours in a warm place..

After decanting the supernatant liquor through a filter, the precipitate, which cakes and clings to the beaker, is dissolved in hydrochloric acid (1 vol. acid: 1 vol. water), a little ammonium oxalate added, the calcium salt reprecipitated with ammonia and acetic acid again added, the bulk being now kept at 200 c.c. The precipitate, on settling for half-an-hour, should be quite free, *i.e.*, not caking or sticking to the glass; if it does so, the precipitation must be repeated. It is then filtered and treated in the usual way.

By this method as little as 0 20 per cent. of CaO can be estimated with ease, and duplicates only vary by 0·02 to 0·03 per cent. The essential points are: (1) Precipitation in presence of acetic acid; (2) employment of a dilute solution, the concentration of which should not exceed one per cent. of magnesium oxide; (3) allowance of ample time for precipitation (between 20 and 30 hours).

NOTE ON THE INFLUENCE OF INCOMBUSTIBLE SUBSTANCES ON COAL-DUST EXPLOSIONS

By A S Blatchford B Sc

[Read March 4th 1915]

Experiments were undertaken in development of Prof Bedson's previous work to observe the quenching effect of different substances to find the most efficient of these and to arrive at a possible explanation of the preventive action Mixtures of coal dust and quenching substance were fired at a constant temperature and the table records the least percentage of quenching substance in a mixture which prevents an explosion

	Coal A	Coal B	Coal C	Coal D
Boiler Ashes	57	50	47	58 60
Quick Lime	50	46	42 44	55
Chance's Mud	38 40	30 33	29 30	39 40
Gypsum	33 35	26 28	26	35
Magnesia	28 30	28	25 26	32 33
Magnesia Alba	22	17 19	15	22 23
Anhydrous Sodium Carbonate	12 13	10+	12	15
Soda Crystals	10	10	9+	11
Sodium Bicarbonate	9 10	7	7	8
Glauber's Salts	8	8	7	8

It was shown that the quenching effect was not due to (1) liberation of carbon dioxide from the decomposition of a quench nor to (2) the influence of water of crystallisation but was due to (3) the prevention of a rapid rise of temperature of a mixture by heat absorption arising from the specific heat and thermo-chemical requirements of the quench

THE RATE OF LIBERATION OF HYDROCYANIC ACID FROM GENETICALLY PURE LINSEED.

By S H COLLINS, M Sc F I C, and H BLAIR

[Read March 9th, 1915]

On a former occasion[1] the authors were able to place before this Society a note on some results obtained when commercial kinds of linseed were digested with water at 45° C, and the yield of hydrocyanic acid measured at intervals of time On that occasion we expressed the hope that we might obtain results from linseeds grown under conditions that would represent but one variety of linseed and not the mixed and hybridised seed of commerce

Our researches have been interrupted by the war but we are now able to report on such work as we have been able to finish

Owing to the kindness of Prof Bateson we have examined three pure strains of linseed with the results of Table I, whilst with the help of Dr J Vargas Eyre we have examined four other pure strains as shown in Table II

The range of yields of hydrocyanic acid vis —166 to 450 per 1,000 are similar to the range of results previously referred to[1], but, on the whole, are higher The range of velocities of reaction, that is from 47 to 83 minutes for yielding half the total amount, are also similar

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[1] See 'The Rate of Evolution of Hydrocyanic Acid from Linseed under Digestive Conditions, *Univ Dur Phil Soc*, vol iv p 99, "The Rate of Liberation of Hydrocyanic Acid from Linseed," *Analyst*, 1914, p 70, "The Rate of Liberation of Hydrocyanic Acid from Commercial Kinds of Linseed,' *Univ Dur Phil Soc*, vol v, p 202, and "The feeding of Linseed to Calves," *Journal of the Board of Agriculture*, vol xxii, p 120

Up to the present there is no evidence that pure strains will yield any result markedly different from commercial kinds of seed. The dwarf blue flower, however, gives results likely to produce a linseed especially suitable for calf feeding owing to its low cyanogen content and to the slow rate at which it comes off.

TABLE I

	Hydrocyanic Acid parts per 1 000	Minutes to evolve one half total amount
Tall blue flower	230	50
White flower	282	53
Dwarf blue flower	166	75

TABLE II

	Hydrocyanic Acid parts per 1 000	Minutes to evolve one half total amount
Seed A	450	47
F	380	82
G	360	55
H	300	83

The Formation and Decomposition of the Hydrides of the Non-metals.

By J. A. Smythe, Ph.D., D.Sc.

[Read February 11th, 1915.]

One of the most fruitful subjects of study in the field of inorganic chemistry during recent years has been that of the binary compounds of the metals and non-metals, especially the nitrides, borides, sulphides, carbides and silicides, the stimulus coming, very largely, from the invention of the electric furnace. Along with the progress of investigation of these compounds, the examination of their reactions, particularly those which lead to the formation of non-metallic hydrides, has claimed much attention, and the results so far obtained have proved of considerable scientific interest and practical utility. Though much yet remains to be accomplished, there is already a mass of scattered information on the subject which, when correlated, gives promise of connecting many, apparently diverse, phenomena, and may lead to a deeper insight into the mutual relations of these compounds. A connected account of the chief results thus far obtained and some of the conclusions which may be, tentatively at least, drawn from them may thus be acceptable at this stage. Particular attention will be directed at first to the chemistry of the non-metallic hydrides, which are the products of the action of water or dilute acids on the binary metallic compounds; later, the best known and most important group of the last-named compounds, viz., the carbides, will claim special treatment. The hydrides will now be considered briefly in their natural groups.

The Halogens form the hydrides FH, ClH, BrH, IH; no other hydrides are known. Hydrogen fluoride is distinguished by its molecular complexity at low temperatures (in the region of the boiling point, 19°), association taking place as illustrated by the equilibrium-equation[1]:

$$F_4H_4 \rightleftharpoons 4FH.$$

This property, affecting the gaseous state, is apparently unique among the hydrides. Polymerisation in the liquid and solid state, presumably accompanied by change in molecular weight, is, however, frequently encountered among the hydrides of carbon, *e.g.*, styrol, cyclopentadien.[2]

The *Oxygen* family form the hydrides OH_2, SH_2, SeH_2, TeH_2, all of which are dissociated into their elements with rise of temperature, those of high molecular weight most easily. Oxygen forms an additional hydride, O_2H_2, readily decomposed into oxygen and water; other hydrides have been postulated, *e.g.*, O_3H_2, by Berthelot, O_4H_2 by Bach, but their existence has been disproved by Baeyer.[3]

Hydrides of sulphur appear to be numerous; two, besides hydrogen sulphide, have been isolated, viz., S_2H_2 and S_3H_2,[4] and there are good grounds for thinking that S_4H_2 and S_5H_2 are capable of existing and possibly also S_6H_2 and S_7H_2. Direct decomposition of the trisulphide into the disulphide and sulphur, and of the disulphide into the monosulphide and sulphur has been proved, and it is probable that the higher polysulphides behave in a similar way on heating.

The *Nitrogen* family form the hydrides NH_3, PH_3, AsH_3, SbH_3; two additional hydrides of nitrogen are known, hydrazine, N_2H_4, and hydrazoic acid, N_3H (leaving out of account the two derivative hydrides, ammonium hydrazoate, N_4H_4, and hydrazine hydrazoate, N_5H_5). Phosphorus yields a liquid hydride, P_2H_4,[5] and several solid hydrides. One of these has the empirical formula, P_2H, but the cryoscopic determination of its molecular weight proves its molecular formula to be $P_{12}H_6$.[6] The liquid hydride is decomposed spontaneously (in contact with hydrochloric acid or calcium chloride) into phosphine and this solid hydride, as expressed in the simplest terms by the equation:

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[1] Thorpe and Hamby, *Soc.*, 1889, lv., 163.

[2] Kronstein, *Ber.*, 1902, xxxv., 4150, 4153.

[3] Baeyer, *Ber.*, 1900, xxxiii., 2488.

[4] Bloch and Höhn, *Ber.*, 1908, xli., 1961, 1971, 1975.

[5] Gattermann and Hausknecht, *Ber.*, 1890, xxiii., 1174.

$$5\,P_2H_4 = 6\,PH_3 + 2\,P_2H.^5$$

When the solid hydride is treated with liquid ammonia or heated in vacuo, it is decomposed into solid and gaseous hydrides as follows:[7]

$$5\,P_2H = P_9H_2 + PH_3;$$

still another solid hydride, P_5H_2, has been described as resulting from the thermal decomposition of P_9H_2, and by the action of acetic acid on the alkali pentaphosphides.[8]

No definite hydride of arsenic other than arsine has been isolated, though a solid hydride, AsH, has been described, but its existence is doubtful. In the case of antimony, too, there is no evidence of hydrides other than stibine, though such have been carefully looked for among the decomposition-products of that compound.[9]

Boron stands, in a sense, apart from the other non-metals, and the recent brilliant investigations of A. Stock and his collaborators[10] have revolutionised our ideas on the chemical relationships of the element. The main results of this work may be briefly summarised as follows: Many well defined hydrides have been isolated, *e.g.*, B_2H_6, B_4H_{10}, $B_{10}H_{14}$, B_6H_{12}, (the last is somewhat uncertain with respect to its hydrogen-content), but no evidence has been obtained of the compound BH_3, corresponding in composition to the chloride and methide and to the position of the element in the periodic classification. So far as the composition of these hydrides is concerned, there is thus a strong, and unexpected, resemblance between boron and carbon, recalling that which exists between other elements of low atomic weight belonging to different groups, *e.g.*, fluorine and oxygen; beryllium and aluminium; lithium and calcium.

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[6] Schenck and Buck, *Ber.*, 1904, xxxvii., 915.

[7] Stock, Böttcher and Lenger, *Ber.*, 1909, xlii., 2839, 2847.

[8] Hackspill, *Compt. rend.*, 1913, clvi., 1466.

[9] Stock and Doht, *Ber.*, 1901, xxxiv., 2339; 1902, xxxv., 2270. Stock and Guttmann, *Ber.*, 1904, xxxvii., 885.

[10] Stock and Massenez, *Ber.*, 1912, xlv., 3539; Stock and Friederici, *Ber.*, 1913, xlvi., 1959; Stock and Priess, *Ber.*, 1913, xlvi., 3353.

Besides the hydrides mentioned above, there are others, both liquid and solid, but less well defined, and the relations subsisting among all these hydrides resemble, in an enhanced degree, those already noted in the case of the hydrides of phosphorus. Thus B_4H_{10} is decomposed spontaneously into hydrogen, B_2H_6 and solid and liquid hydrides, in which the mean atomic ratio of boron to hydrogen is 1 : 1·2. B_2H_6 again, though more stable than B_4H_{10}, suffers decomposition when sparked, or heated, or even under the influence of ultraviolet light, into $B_{10}H_{14}$ and other hydrides. In the words of Stock, "Ein so leichtes Uebergehen von Wasserstoffverbindungen in andere, teils niedriger, teils höher molekulare, bei Zimmertemperatur, ist eine bisher unbekannte Erscheinung."

Carbon and Silicon, the two remaining elements to be considered, form the corresponding hydrides CH_4, SiH_4 and C_2H_6, Si_2H_6. The last of these is generated, along with the tetrahydride, by the action of hydrochloric acid on magnesium silicide[11] or, better, from lithium silicide; there is also evidence of a lower hydride, Si_2H_4, and of a solid hydride of unknown composition. Several ill-defined hydrides appear also to be formed by the decomposition of the tetrahydride. The number of hydrides of carbon is legion, and many, too, are the methods used in their preparation; but the only aspects of the subject which concern us, in this place, are the preparation of these hydrides, the hydrocarbons, from the carbides and their conversion into one another by the agency of heat.

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[11] Lebeau, *Compt. rend.*, 1909, xlviii., 43.

THE FORMATION OF HYDROCARBONS FROM CARBIDES.

The carbides of the metals can be classified according to their reactions with water and dilute acids; some are unattacked, others, like calcium carbide, yield acetylene; aluminium carbide gives methane, and the carbides of iron, thorium, uranium and the cerium group yield a complex mixture of gaseous, liquid and, in cases, solid hydrocarbons, both saturated and unsaturated, belonging to the paraffin series. Precise details of these reactions will be given later; meanwhile, a point of resemblance between the carbides and the phosphides, borides, silicides and, possibly, the polysulphides may be noted, in that the hydrides derived from many of these compounds are characterised by great complexity of mixture—most marked in the case of carbon in accordance with the stability of the hydrocarbons.

In seeking for an explanation of this phenomenon, several possibilities present themselves.

1. The binary metallic compounds may be mixtures, each component yielding a corresponding hydride on reaction. This was the view entertained by Stock and Messener (*op. cit.*), who write, with reference to magnesium boride: "Offenbar ist das sogennante Magnesiumborid eine kompliziert zusammengesetzte Substanz, und seine Zersetzung durst Wasser und Säuren ein höchst verwickelter Vorgang." Yet the proof, adduced by the authors themselves, of the ease of decomposition of the hydrides of boron, not merely at the moment of liberation, when experience shows a general exaltation of activity, but in a prepared condition, renders such an explanation quite superfluous.

Against the view in general it may be urged that these compounds are usually crystalline and well-characterised (many carbides, silicides and borides have been investigated crystallographically), and that efforts have been made in individual cases to separate possible constituents of admixture, though without success. Thus, referring to the carbide of iron, Mylius and his collaborators[12] say: "Die chemische Individualitat des Carbids wird bewiesen durch die fractionierte Lösung der Substanz welche die Zusammensetzung derselben nicht andert"; and Moissan,[13] led by the observation that cerium carbide evolves, with water, both acetylene and methane, just as though it were a mixture of two carbides like those of calcium and aluminium, fractionally decomposed the compound with water, but without bringing about any alteration in the composition of the derived gases. Furthermore, to account for the reactions of, say, uranium carbide, on this view, one would have to assume this body to be a mixture of dozens of carbides, some with, perhaps, twenty or thirty carbon atoms in the molecule; such an assumption has, clearly, nothing to recommend it.

2. The metallic compounds, though of simple empirical composition, may be molecularly complex and so may yield, on reaction, many products, some of high, others of low, molecular weight; just as, for example, dibenzyl, of the empirical formula CH, gives on chlorination such derivatives as $C_{14}H_{12}Cl_2$, C_6Cl_6, and C_2Cl_6, in virtue of its high molecular weight (the molecular formula being $C_{14}H_{14}$) and the molecular disruption which accompanies exhaustive reaction.

Now, on this question of the molecular complexity of the compounds under discussion, nothing definite can be stated, as methods for determining their molecular weight have not yet been discovered. There would, however, appear to be no advantage in assuming a high molecular weight for some carbides and not for others. Looking at the problem in its general aspect, the molecular simplicity of the solid state has often been affirmed by chemists, though possibly the prevalent views are to the contrary;

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[12] Mylius, Foerster and Schoene, *Ber.*, 1896, xxix., 2991.

[13] Moissan, *Compt. rend.*, 1896, cxxii., 357.

thus van t'Hoff[14] was led, especially by the study of the phenomena of solid solutions, to the conclusion "dass der feste Zustand nicht in hochmolekularer konstitution seine Ursache hat." Further, it may be noted that the effect of heat is invariably to promote the dissociation of complex molecules, and it may thus be inferred that substances made in the electric furnace are likely to have low molecular weights.

Collateral evidence on the point, from the chemical side, is afforded by the action of chlorine on silicon and the silicides. It has been shown that silicon, freed from magnesium silicide, yields only the tetrachloride $SiCl_4$, by reaction with chlorine, but that higher chlorides, Si_2Cl_6 and Si_3Cl_8, are produced when the silicide is not removed;[15] moreover, that ferro-silicon, containing 50 per cent. of silicon, gives good yields of these higher chlorides, and that the hexachloride is not reduced to the tetrachloride by means of silicon.[16] The inference to be drawn from these observations is that silicon is molecularly simple (monatomic) and that the silicides probably contain the groupings Si_2 and Si_3; but as silicides of corresponding empirical composition have been isolated,[17] it may be argued that their empirical formulæ are also molecular and, therefore, that the silicides are compounds of low molecular weight.

3. Though the arguments so far adduced cannot be regarded as decisive, yet their cumulative strength is against the idea that the complex mixture of hydrides, resulting from the aqueous decomposition of many carbides and similar compounds, can be accounted for by the molecular complexity of these compounds, or by the supposition that a mixture is masquerading as an individual. An explanation of the phenomenon, free from the objections

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[14] van t'Hoff, *Vorlesungen über theoretische und physikalische chemie*, 1899, vol. ii., 73.

[15] Gattermann and Ellery, *Ber.*, 1899, xxxii., 1114.

[16] Martin, *Soc.*, 1914, cvi., 2836.

[17] Burton, *Univ. Durham Phil. Soc.*, 1910, vol. iii., part 5, 293.

which can be raised against these, is suggested by the ease and character of the decomposition of the hydrides of boron (and, in a less degree, those of phosphorus and silicon too). Stated briefly, the suggestion is that the complex hydride-mixture, produced in many cases by the reaction of carbides, borides, silicides and phosphides with water, is the result of the decomposition of a single hydride, the unique gaseous product of the double decomposition, corresponding in composition to the metallic compound from which it is derived. It may be asked, however, whether the important group of the hydrides of carbon shows a behaviour similar to that of the other hydrides in question? The answer is that spontaneous decomposition, such as the hydride of boron, B_4H_{10}, exhibits, is unknown among the hydrocarbons, but the thermal decomposition of other hydrides above mentioned is a highly characteristic property of the hydrocarbons and is indeed employed in practice for the production of light oils from heavy oils, the operation being known as "cracking." The main features of this process must now be considered.

THE DECOMPOSITION OF HYDROCARBONS.

Almost all natural occurrences of hydrocarbons are extremely complex mixtures belonging to certain well-known classes of carbon compounds (paraffins, both saturated and unsaturated, naphthenes, etc.); similarly, artificial hydrocarbon-products such as arise from the destructive distillation of bitumen, coal, oil-shales, etc., are characterised by a like complexity of mixture. It is found, experimentally, that when any hydrocarbon is raised to the temperature at which it is decomposed, a number of new hydrocarbons is produced, some of lower, others of higher boiling point than the original—corresponding, in general, to less or greater molecular complexity. Many attempts have been made to trace the individual steps in the progress of these reactions. Thus the primary decomposition by heat of butane is stated to result in the formation of ethane and ethylene,[18] and normal hexane is resolved into amylene and methane.[19] A simpler example is that of ethylene which, at 800°, yields methane and acetylene, according to the equation[20]:—

$$3\ C_2H_4 = 2\ CH_4 + 2\ C_2H_2.$$

The validity of this equation has, however, been called into question.[21]

Now such reactions must be looked upon as ideal, and only, even partly, realisable by selection of special experimental conditions, which enable the primary products of the cracking process to be quickly removed. In all ordinary cases, these products themselves suffer similar changes of decomposition, *and at the same time other reactions* set in; unsaturated hydrocarbons are polymerised, and reactions of condensation (using the term in the sense of combination of two or more molecules with loss of hydrogen) and addition (particularly hydrogenation) take place, whereby more complex hydrocarbons are produced. For example, in the thermal decomposition of acetylene, this compound is polymerised to benzene; acetylene and benzene condense to form naphthalene, with elimination of hydrogen, and a reaction of addition between naphthalene and acetylene yields acenaphthene.[22] Thus, along with the resolution of complex hydrocarbons into simpler ones (cracking, in the literal sense of the word) there proceeds the reverse synthetic process, and as the temperature-intervals favourable to many of the reactions overlap, it follows that great complexity of products is the result, and the extent to which control is possible is restricted to the preservation of the optimum-temperature (and pressure) for a particular reaction or, rather, set of reactions.

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[18] Thorpe and Young, *Proc. Roy. Soc.*, 1873, xxi., 184.

[19] Haber, *Ber.*, 1896, xxix., 2691.

[20] Lewes, *J. Soc. Chem. Ind.*, 1892, 584.

[21] Bone and Coward, *Soc.*, 1908, xciii., 1197.

[22] R. Meyer, *Ber.*, 1912, xlv., 1609; R. Meyer and Tanzen, *Ber.*, 1913, xlvi., 3183.

Several comprehensive investigations of this character have been carried out in recent years; among these may be cited the repetition, on a large scale, of Berthelot's work on the thermal decomposition of acetylene,[22] and the detailed study of the cracking of hexane, hexamethylene, amylene and ethylene at various temperatures and, even in the cold, in presence of a catalyst and under high pressure.[23] All of these processes yield a highly complex mixture of gaseous, liquid and solid hydrocarbons, both saturated and unsaturated, and hydrogen is an invariable constituent of the gases. One result of considerable interest, which may be mentioned in passing, is the proof that it is possible, by selection of conditions, to prepare from hydrocarbons like ethylene or amylene, liquid hydrocarbon-mixtures resembling closely the three chief types of natural rock-oils, viz., those from America, Baku, and Galicia.*

From the point of view of the present enquiry, the phenomena of the thermal decomposition or cracking (to apply the technical term to embrace both the analytic and synthetic reactions which take place) of hydrocarbons are of importance, in that they bear witness to the ease of decomposition of the hydrides of carbon, and the formation from individual hydrides of a complex mixture, some containing more hydrogen, others less, than the original; and it is reasonable to associate this behaviour with the phenomena attending the liberation of hydrocarbons from carbides; of the hydrides of phosphorus, boron and silicon from their metallic representatives; and with the decomposition, both thermal and spontaneous, of the hydrides of boron. That such analogies have not been entirely overlooked is evident from the words of Moissan[24] who, speaking of the decomposition of uranium carbide by water, says: "Il est vraisemblable que cette décomposition complexe tiènt à des phénomènes de polymérisation, analogues à ceux que M. Berthelot a décrit dans ses recherches sur la décomposition pyrogénée des carbures d'hydrogène."

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[23] O. Aschan, *Annalen*, 1902, cccxxiv., 23; Engler and Rontala, *Ber.*, 1909, xlii., 4613; Ipatiew, *Ber.*, 1911, xliv., 2978; Ipatiew and Dowgelewitsch, *ibid.*, 2987; Ipatiew and Rontala, *Ber.*, 1913, xlvi., 1748; see also C. Engler, "Die neueren Ansichten über die Entstehung des Erdoles," 1907.

* Like so many geo-chemical syntheses, these do not give the clue to the origin of the natural products, though the information is naturally of great value in the study of the subject. It may be noted that the formation of hydrocarbon mixtures from carbides and water led Moissan to the formulation of his well-known theory of the origin of rock-oil.

The main conclusions so far arrived at may now be briefly summed up. It is probable that the binary metallic compounds, from which the hydrides of phosphorus, boron, carbon and silicon are derived, are, in general, individuals of low molecular weight, corresponding to their empirical formulæ; that the primary reactions, whereby the hydrides are produced from them, are simple; and that complexity of products, when this arises, is the result of a series of reactions, exhibited by all the hydrides in a prepared state, but best illustrated by the thermal decomposition of the hydrocarbons, and which, taking place at the ordinary temperature, may by analogy be referred to as reactions of "cold cracking."

Review of the Hydrides.

From the facts already set forth, it is apparent that the hydrides fall into three classes, according to their behaviour on heating and the phenomena attending their formation. These properties appear to depend, in some degree, on the heats of formation of the compounds.

1. The hydrides of the halogens, water, hydrogen sulphide, and methane are stable in character, but dissociate into their constituent elements on heating; all are exothermal except hydrogen iodide, which is feebly endothermal at moderate temperatures but becomes exothermal above 320°.

2. Hydrogen peroxide, the persulphides of hydrogen, hydrazoic acid and stibine represent the unstable hydrides which are decomposed readily, often explosively, into their elements or more stable hydrides. Some of these are known

to be strongly endothermal, *e.g.*, $O,OH_2 = -23$; $N_3,H = -62$; Sb,H_3[25] $= -34$ cals.

3. The third class includes many of the hydrides of phosphorus, boron, silicon and the hydrocarbons other than methane, and the members of this class exhibit the phenomenon of cold-cracking and also, in the prepared state, of ordinary cracking, or thermal decomposition with its accompanying synthetic reactions. Only in a few cases is the heat of formation of these hydrides known ($C_2,H_4 = -2{\cdot}7$; $C_2,H_2 = -47{\cdot}8$ cals.), but it is probable that most of them are endothermal, corresponding to the increased proportion of the acidic element (compare ammonia and hydrazoic acid or ethylene and acetylene in this respect) and, in the case of silicon, to its position in the carbon group, rise of atomic weight carrying with it decrease in the affinity of the element for hydrogen (compare ammonia and stibine). It seems, indeed, that the peculiarities of this class depend first upon the high valence of the acidic element, which determines the structural possibility of many compounds with uni-valent elements, and secondly, on the endothermal nature, presumably not very pronounced, of the hydrides, which conditions limited stability and the possibility of decomposition stopping short of completeness, *i.e.*, resolution into constituent elements. The various products of cracking may thus be looked upon as halting stages, as pictured by the law of successive reactions, in the degradation of the energy of the endothermal hydrides.

That these hydrides, at the moment of liberation, should spontaneously undergo decomposition similar to that produced by heating them in the prepared state, is in harmony with experience relative to the activity of elements in what is commonly called the "nascent" state,* and it is of interest

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[24] Moissan, *Compt. rend.*, 1896, cxxii., 279.

[25] Stock and Wrede, *Ber.*, 1908, xli., 540.

* It is unfortunate that this term has been used to connote a condition of activity dependent on an assumed difference in atomic or molecular composition; the energy-conditions of an element in the nascent and ordinary, or prepared, state are so different that no such inference can be drawn.

to note that such reactions are very susceptible to change of condition, which would obviously lead to alteration in the distribution of available energy. Thus Moissan[26] found that ice-cold water generated, by reaction with cerium carbide, a gas appreciably richer in acetylene, and correspondingly poorer in ethylene and methane, than water at the ordinary temperature, and that the yields of acetylene from this carbide by treatment with water, hydrochloric acid and nitric acid (both acids exceedingly dilute) varied considerably, being 71, 65·8 and 83 per cent. by volume.

Again, it is found, in the preparation of acetylene from calcium carbide, that if the temperature be allowed to rise to 100°, the yield of gas is greatly diminished, a portion of the hydrocarbon becoming polymerised.[27] In each case, then, the effect of a slight rise of temperature on the materials from which acetylene is being generated, is to reduce the yield of that gas, that is, to promote its decomposition, and as, in the prepared condition, such changes only begin at 422° (the optimum-temperature is 640-50°; see Richard Meyer, *op. cit.*), it would seem that a trifling rise of temperature has much the same effect on "nascent" acetylene as a rise of some 400° has on the ready-made gas.

It is not difficult to understand, now, why the carbides should show such variation in their reactions with water. When acetylene or some similar hydrocarbon is the primary product of reaction, and the energy-conditions of the environment are favourable, this gas may be evolved as such; under other conditions it may be forthwith cracked, with resulting formation of a complex hydrocarbon mixture, and it is noteworthy that, in such cases, acetylene and ethylene are always found among the gaseous products, and the liquid hydrocarbons are partly unsaturated. When, however, methane is the first resultant of reaction, it is preserved by reason of its great stability; cracking in this case could result only in carbon and hydrogen. Though the presence of hydrogen in the gases yielded by the carbides has been referred either to reaction with water of the "excess of metal" in the compound,[28] or to secondary decomposition of water by the lower oxide of the metal primarily formed,[29] and though it has usually been held that the saturated hydrocarbons in these gases arise by the reduction of unsaturated hydrocarbons by means of this hydrogen,[30] yet there seems as much probability that both the hydrogen and the saturated hydrocarbons are the products of the cracking of the acetylene or similar compounds first formed.

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[26] Moissan, *Compt. rend.*, 1896, cxxii., 357.

[27] Le Chatelier, *Leçons sur le Carbone*, p. 152.

The nature of the decomposition-products of the carbides with water is so remarkable as to direct attention very forcibly to the carbides themselves. It may be well, therefore, in concluding this study of the hydrides, to describe the carbides more in detail and to see what conclusions can be legitimately drawn from their reactions as to their nature and constitution.

THE CARBIDES.

Our knowledge of these compounds is due, in great measure, to the researches of Moissan [31] carried out during the decade 1893-1903. They are stable, high-temperature compounds and are generally prepared in the electric furnace. A distinction may be made between the carbides proper and the acetylides, which are unstable compounds prepared in the wet way by means of acetylene; the carbides of rubidium, caesium and magnesium form, perhaps, a connecting link between these two classes. Not all of the metals are able to form carbides, some dissolving carbon when molten and rejecting it on solidification. The following table comprises the well-investigated carbides, arranged in accordance with the position of the elements in the periodic classification. Carbides of neodymium, praesodymium, samarium, ytterbium and holmium have been prepared, but these rare-earth metals find no position in the ordinary scheme of classification adopted here; the composition of the carbides of rubidium, caesium and magnesium seems doubtful, so that these are omitted from the table. Where the members of a series do not form carbides, the series is omitted. The only carbides missing from the table (leaving group VIII out of consideration) are those of rubidium, caesium, magnesium, scandium, ytterbium, niobium and tantalum; many of these metals are of very rare occurrence and have been but slightly investigated.

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[28] Berthelot, *Compt. rend.*, 1901, cxxxii., 181. It is not quite clear from Berthelot's statement, whether this excess of metal is to be regarded as free or combined. It may be added, that though in the case of manganese carbide, Mn_3C, one might regard the metal as being in excess, that is hardly justifiable in the case of thorium carbide, ThC_2, and yet the gases obtained from this carbide contain, according to Moissan, 16 per cent. of hydrogen.

[29] Moissan, *Compt. rend.*, 1896, cxxii., 279.

[30] Berthelot, *op. cit.*; also E. D. Campbell, *Amer. Chem. Journ.*, 1896, xviii., [illegible]6.

[31] See especially H. Moissan, *Le four électrique.*

Group.	I	II	III	IV	V	VI	VII	VIII
Series 2	Li_2C_2	Be_2C	$\{B_6C\}$	CARBON	—	—	—	—
,, 3	Na_2C_2	—	Al_4C_3	$\{SiC\}$	—	—	—	—
,, 4	K_2C_2	CaC_2	—	$\{TiC\}$	$\{VC\}$	$\left\{\begin{matrix}Cr_4C\\Cr_3C_2\end{matrix}\right\}$	Mn_3C	Fe_3C Ni_3C.
,, 6	—	SrC_2	YC_2	$\left\{\begin{matrix}ZrC\\ZrC_2\end{matrix}\right\}$	—	$\left\{\begin{matrix}MoC\\Mo_2C\end{matrix}\right\}$	—	—
,, 8	—	BaC_2	LaC_2	CeC_2	—	—	—	—
,, 10	—	—	—	—	—	$\left\{\begin{matrix}W_2C\\WC\end{matrix}\right\}$	—	—
,, 12	—	—	—	ThC_2	—	U_2C_3	—	—

It will be seen from this table that the metals (and the quasi-metals, boron and silicon) which form carbides belong to the two short periods (series 2 and 3), and the even series of the long periods. The non-existence of carbides of the metals belonging to the odd series of the long periods does not evidently arise from any inability of these metals to combine with carbon, since they unite with the hydrocarbon radicals to form the organo-metallic compounds, in which direct union of metal with carbon is inevitable. It is a curious point, upon which little can as yet be said, that the metals of the two short series combine with carbon both as carbide and *organo-metallic* derivative, while of the long periods, only the metals of the even series form carbides and only the metals of the odd series form organo-metallic compounds.

With respect to the action of water and dilute acids, there is evidently a rough connexion between the behaviour of a carbide and its position in the table All the carbides in brackets { } are unattacked by water; those of the first and second groups, except beryllium carbide, yield with water acetylene only; beryllium and aluminium carbides yield pure methane; manganese carbide, a mixture of methane and hydrogen in equal volumes; yttrium, lanthanum, cerium, and the carbides of the other rare earths, also those of thorium, uranium and iron, yield a complex mixture of gaseous, liquid and, sometimes, solid hydrocarbons. The composition of the gases evolved from some of these is given by Moissan as follows:—

	YC_2	LaC_2	CeC_2	PrC_2	NdC_2	SaC_2	ThC_2	U_2C_3
C_2H_2 ..	72	70	75	67 5	66	70	47	0 2
CH_4 ..	19	28	21	30	27 5	22	31	78
C_2H_4 ..	4	2	4	2 5	6 5	8	6	6 8
H_2 ..	5	—	—	—	—	--	16	15

More recent investigation, while not confirming these analytical results in detail, has only served to emphasise the complexity of the gaseous mixture. Thus Damiens[32] states that the carbides of the cerium group yield acetylene and allylene as the chief products, along with small quantities of ethylene, ethane, and their homologues, a little hydrogen, but no methane; while the carbides of thorium and uranium[33] give, as chief products, hydrogen, methane, ethane, propane, butane, and, in addition, ethylene and its homologues and some acetylene. The liquid hydrocarbon-products do not seem to have been carefully examined, but it is stated that two-thirds of the carbon of uranium carbide passes into this form and the liquid boils from 70 to 200°, leaving a bituminous residue.[34]

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[32] Damiens, *Compt. rend.*, 1913, clvii., 214.

[33] Lebeau and Damiens, *Compt. rend.*, 1913, clvi., 1987.

The carbide of iron is hardly attacked by water, in the absence of air, below 100°; it is, however, completely soluble in hydrochloric acid and the gas evolved contains 92 per cent. of hydrogen, the remainder being a hydrocarbon-mixture of the mean density of pentane.[35] According to the more accurate determinations of Campbell,[36] the gas given off contains 84 per cent. of hydrogen and the remaining 16 per cent. consists of paraffins and olefines, 40 per cent. of the carbon in the carbide passing into the liquid products. Mendeléeff[37] states that the liquid hydrocarbons derived by dissolving the crude carbide (pig iron) in acid are similar in taste, smell and reaction to natural naphtha, and Hahn and Cloez identified among the unsaturated hydrocarbons, produced in this manner, ethylene, propylene and butylene, by conversion into their addition compounds with bromine.

The extraordinary difference in behaviour exhibited by the carbides in their reaction with water and the unexpected complexity of the products, in many cases, have arrested the attention of several chemists Berthelot[38] has shown, from the thermal study of some of the reactions, that the composition of the gases evolved corresponds to the greatest loss of heat, or, in other words, that the reactions are in accordance with the principle of maximum work. This result, interesting though it is, does not give any explanation why such differences should exist; for that we require to know something of what, for want of a better name, we may call the constitution of the carbides. This term must be understood as referring rather to the existence of certain atomic groupings in the molecule, than of structure conform-

[34] Moissan, *Compt. rend.*, 1896, cxxii., 274.

[35] Mylius, Foerster and Schoene, *Ber.*, 1896, xxix., 2991.

[36] Campbell, *Amer. Chem. Journ.*, 1896, xviii., 836.

[37] Mendeléeff, *Principles of Chemistry*, vol. i., p. 365.

[38] Berthelot, *Compt. rend.*, 1901, cxxxii., 181.

known hydrocarbon, C_2H_{12}. The primary decomposition of this hydrocarbon might take place in three ways, giving $C_2H_6 + 3H_2$; $C_2H_4 + 4H_2$, and $C_2H_2 + 5H_2$, the corresponding yields of hydrogen being 75, 80 and 83 per cent. by volume. The yield found in Campbell's experiments is 84 per cent.: but this agreement is certainly accidental, as it takes no heed of the liquid products, nor of the possible production of hydrogen by the decomposition of the first-liberated hydrocarbon.

Continuing this line of argument, it is evident that the constitution of the three similarly-composed carbides, CaC_2, CeC_2 and ThC_2, is not the same, for the action of water yields oxides (or hydroxides) of different composition, viz.: CaO, Ce_2O_3,[41] ThO_2. Allocating equivalent amounts of oxygen and hydrogen to the metal and carbon respectively, the three carbides thus appear as derivatives of the hydrocarbons C_2H_2, C_2H_3 and C_2H_4; the decomposition of the last two would be capable of furnishing the complex mixture which experiment shows to be the result. Similar reasoning would apply in the case of uranium carbide, U_2C_3, which yields the lower, green hydrated oxide when decomposed by water in absence of air.[42]

These results, sketchy and inconclusive as they necessarily are, by reason of our limited knowledge of the reactions of the carbides, yet indicate that, with fuller knowledge, substantial progress may be made in what, at first sight, appears an unpromising subject. The recent studies of the action of nitrogen on some of the carbides, besides being of immense importance from the point of view of the utilisation of atmospheric nitrogen, have also bearings of theoretical interest, as indicated above, and they hold out the promise that other reactions of the carbides will be discovered which may help to unravel the problem of the constitution of these compounds.

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[41] Damiens, *Compt. rend.*, 1913, clvii., 214.

[42] Moissan, *Compt. rend.*, 1896, cxxii., 274.

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4 for 1911-12	£2 0 0	
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2 for 1914-15	1 0 0	
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19 for 1912-13	4 15 0	
69 for 1913-14	30 10 0	
		£80 14 7

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Vol V., Part 3		18 18 6
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On deposit Newcastle Savings Bank	34 2 1	
In hand	2 2 3	
		41 4 4
		£80 14 7

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	£ s. d
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	£50 13 8

		£ s. d.
Balance invested as under —		
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Accrued interest to October 13 1914	3 3 2	
On deposit Newcastle Savings Bank	1 10 6	
		50 13 8
		£50 13 8

Life Composition Fund

	£ s. d
Balance brought forward	40 0 0
	£40 0 0

	£ s. d.
Transferred to Revenue Account	5 0 0
Balance invested in 35 4 % Preference Shares in Grainger Building Society	35 0 0
	£40 0 0

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J W BULLERWELL.

February 4th, 1915.

PROCEEDINGS

OF THE

UNIVERSITY OF DURHAM

PHILOSOPHICAL SOCIETY.

EDITED BY PROF G W TODD M A D Sc

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University of Durham Philosophical Society

THE INDUSTRIAL FIXATION OF NITROGEN

A FEW OBSERVATIONS ON THE HABER PROCESS

GEORGE W TODD M A, D Sc

The fundamental importance of combined nitrogen in agriculture has been fully recognised for a long time, and the late war brought into special prominence its importance in munitions Nitrogen is an essential constituent of all vegetation and the production of food depends greatly on its application to the soil in forms easily assimilated by plants The ever increasing demand for fertilisers (the consumption roughly doubling every ten years) has necessitated additional sources of supply Investigators have naturally turned their attention to the possibility of drawing upon the practically unlimited supply of atmospheric nitrogen

One of the most interesting and most promising methods for the fixation of atmospheric nitrogen is the Haber process In this process the synthesis of ammonia from its elements is effected by passing a suitably proportioned mixture of nitrogen and hydrogen at high pressures—100 to 200 atmospheres—over a catalyst A small percentage of the gases, depending on the conditions of working, is converted into ammonia which may be removed by absorption in water or acid or by condensation at low temperature The process is continuous—the unconverted gases being returned into the circulatory system with a further supply of the mixed gases Many different catalysts effect the synthesis but each demands its own conditions of temperature and pressure

The Haber process is capable of producing ammonia and ammonium sulphate at a low cost and the power requirements per unit of nitrogen fixed are much smaller than those of any other fixation process. The outstanding item in the production costs of synthetic ammonia is hydrogen of the requisite purity.

The reaction taking place between the mixed gases may be represented thus :— $N_2 + 3H_2 \rightleftarrows 2NH_3$.

The velocity of the forward reaction $= k_1 [N_2] [H_2]^3$.

„ „ „ backward „ $= k_2 [NH_3]^2$.

the brackets signifying concentrations. At equilibrium these velocities are equal, *i.e.*,

$\frac{k_2}{k_1} = \frac{[NH_3]^2}{[N_2][H_2]^3}$ = constant for a particular temperature

Haber has experimentally determined this equilibrium constant at many different temperatures and concentrations. Defining the equilibrium constant in terms of the partial pressures of the gases as

$$K = \frac{p_{NH_3}}{p_{N_2}^{\frac{1}{2}} \times p_{H_2}^{\frac{3}{2}}} \text{ he finds that}$$

$$\log_{10} K = \frac{13200}{4{\cdot}571\,T} - 6{\cdot}134.$$

T being the absolute temperature.

If the percentage of ammonia is small and the total pressure is one atmosphere we may take (in a mixture $N_2 + 3H_2$)

$$p_{NH_3} = \left(\frac{1}{4}\right)^{\frac{1}{2}} \times \left(\frac{3}{4}\right)^{\frac{3}{2}} \times K = 0{\cdot}325K.$$

p_{NH_3} being in atmospheres.

Hence if *E* is the equilibrium percentage of ammonia, we have at one atmosphere

$$\log_{10} E = \frac{13200}{4{\cdot}571\,T} - 4{\cdot}622.$$

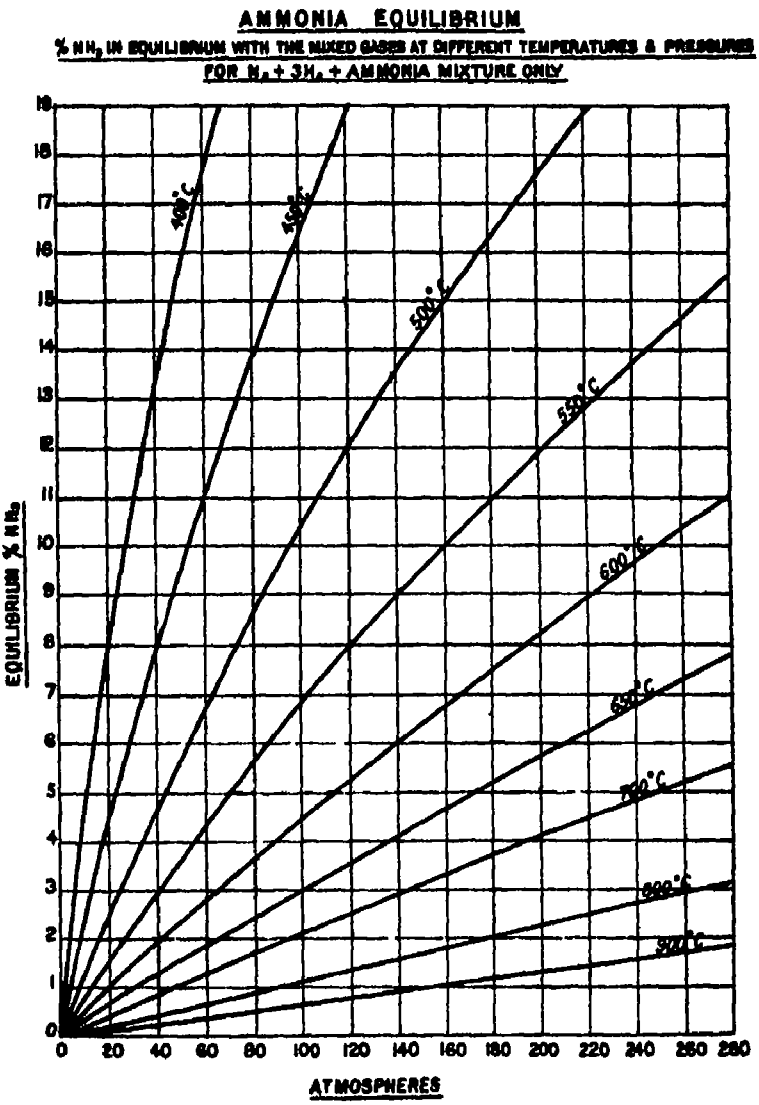

AMMONIA EQUILIBRIUM
% NH_3 IN EQUILIBRIUM WITH THE MIXED GASES AT DIFFERENT TEMPERATURES & PRESSURES
FOR N_2 + $3H_2$ + AMMONIA MIXTURE ONLY
EQUILIBRIUM % NH_3
ATMOSPHERES
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
0
20
40
60
80
100
120
140
160
180
200
220
240
260
280
400°C
450°C
500°C
550°C
600°C
650°C
700°C
800°C
900°C

Fig 1

This expression agrees well with the figures for one atmosphere in the following table due to Haber.

TABLE (Haber. Z.Electroch. 20.600.1914).

t° C.	Per cent. NH in equilibrium at pressures (atm) of			
	1	30	100	200
200	15·3	67·6	80·6	85·8
300	2·18	31·8	52·1	62·8
400	0·44	10·7	25·1	36·3
500	0·129	3·62	10·4	17·6
600	0·049	1·43	4·47	8·25
700	0·0223	0·66	2·14	4·11
800	0·0117	0·35	1·15	2·24
900	0·0069	0·21	0·68	1·34
1,000	0·0044	0·13	0·44	0·87

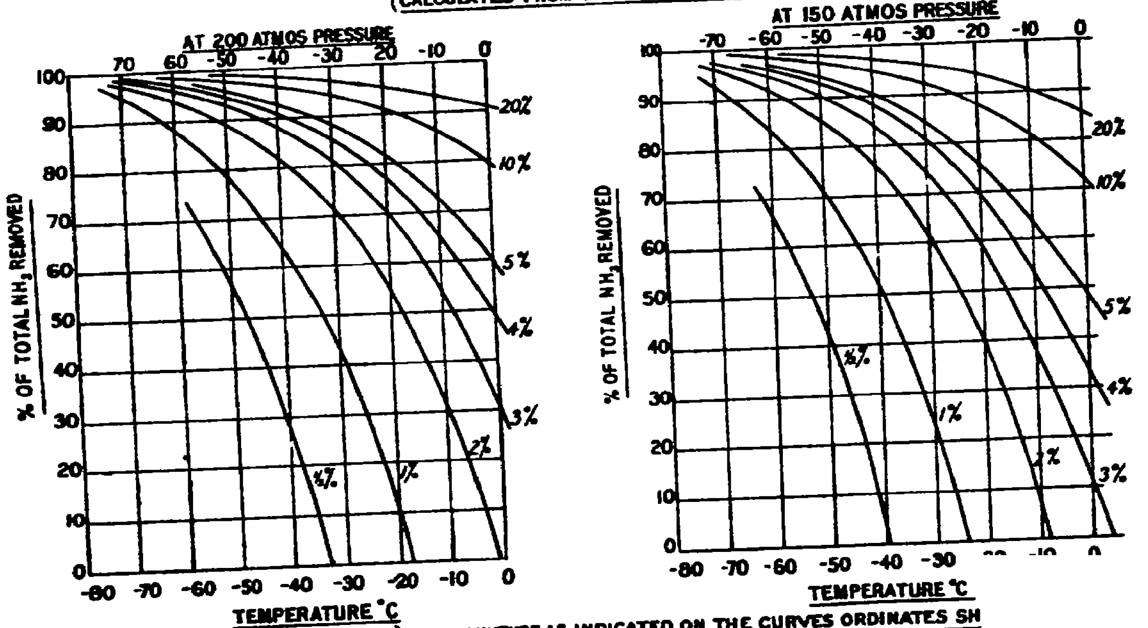
The numbers in the above table are shown plotted in Fig. 1.

We can see at a glance from Fig. 1 what to expect from a given catalyst. In the known large scale Haber plants iron alloys are used as catalysts. These are only active at high temperatures, say in the region above 600° C. Working at a pressure of 200 atmospheres should give at best only 8·2 per cent. ammonia in the mixed gases. The difficulties of working increase enormously as the pressure is increased. It therefore seems desirable to look for low temperature catalysts. A catalyst working at 400° C. would give the same yield at a pressure of only 20 atmospheres and the plant could be simplified and be made much safer to work with. One or two low temperature catalysts are known but apparently they become poisoned and therefore inactive by the most minute traces of impurities in the mixed gases. Very little research however has been done in this direction.

Now a few remarks on the recovery of the ammonia.

The ammonia produced in the catalyst bomb is usually taken away from the mixed gases by absorption. Condensation by cooling offers some attractions. By simple calculations from the saturated vapour pressure curve of liquid ammonia it is possible to determine what percentage of ammonia present in the mixed gases will be condensed at various temperatures and pressures. I have made these

REMOVAL OF NH_3 FROM GASEOUS MIXTURES BY COOLING
(CALCULATED FROM VAPOUR PRESSURE DATA)

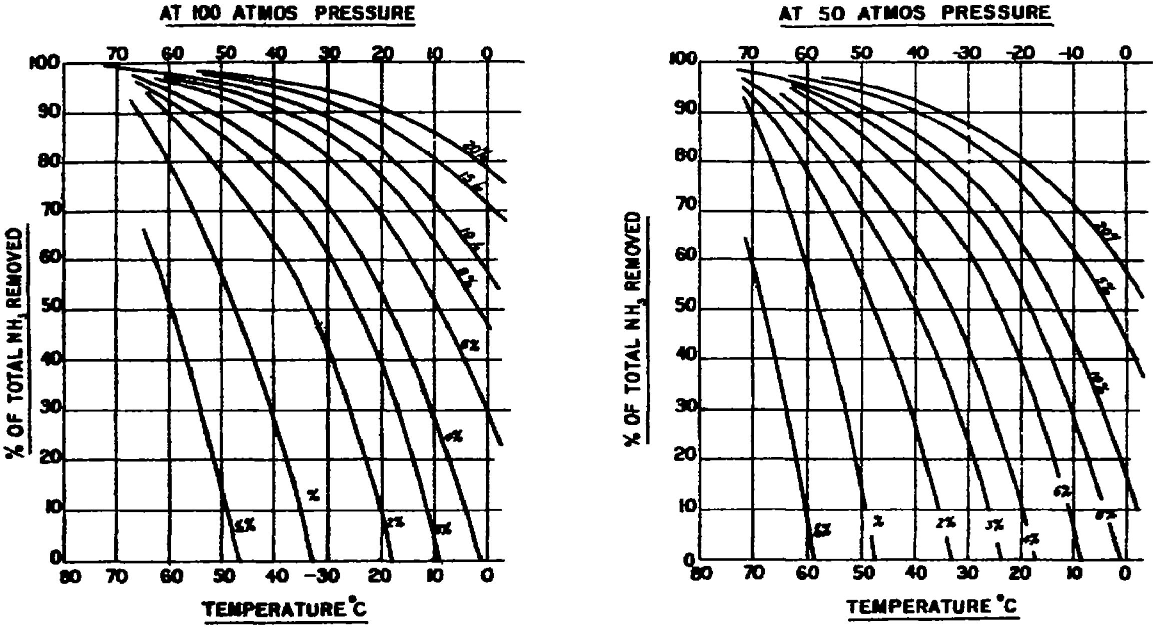


THE CONTENT OF NH_3 (% BY VOL) IN THE MIXTURE IS INDICATED ON THE CURVES ORDINATES SH
CONTENT REMOVED

FIG. 2.

REMOVAL OF NH$_3$ FROM GASEOUS MIXTURES BY COOLING

(CALCULATED FROM VAPOUR PRESSURE DATA)



THE CONTENT OF NH$_3$ IN THE MIXTURE IS SHOWN ON THE CURVES ORDINATES SHOW % OF NH$_3$ CONTENT REMOVED

Fig 3

calculations for four different pressures and the results are shown in Figs. 2 and 3. The graphs show what amount of refrigeration is necessary to yield any desired percentage of the total ammonia present at any of the four pressures taken.

If it were possible to employ a low temperature catalyst the merits of removal by condensation would be very much greater. Take an instance. Fig. 1. shows that a 400° C. catalyst gives a total ammonia content of nearly 16 per cent. at the low pressure of 50 atmospheres; and Fig. 3 shows that nearly 90 per cent. of this ammonia would be condensed out at -10° C. only.

The reaction $N_2+3H_2 \rightarrow 2NH_3$ is exothermic. For every grammolecule of ammonia formed at ordinary temperatures 12,000 calories are given out. The heat of formation increases with temperature. There is thus the possibility of making the process auto-thermic, the temperature of the catalyst being maintained by the exothermic reaction. This point has not been seriously considered. By means of a suitably designed catalyst bomb the outflowing gases could be made to heat the inflowing gases and the temperature of the catalyst could be maintained constant by regulating the rate of gas flow. With a low temperature catalyst and consequently a greater yield of ammonia I have no doubt that the process would be more than autothermic. There would be energy to spare and the cost of production of synthetic ammonia would be very considerably reduced.

The figures in the text have been reproduced from "Physical and Chemical Data of Nitrogen Fixation" (H.M. Stationary Office) with the kind permission of the Munitions Inventions Department.

EXPERIMENTS ON THE FLOW OF GAS AT LOW PRESSURES THROUGH CAPILLARY TUBES.

GEORGE W. TODD, M.A., D.Sc.

It was Clerk Maxwell who deduced from the Kinetic Theory the law that within wide limits of pressure the viscosity of a gas is independent of the pressure. The law has been verified by numerous experiments. When, however, the pressure is reduced until the mean free path of the gas molecules approaches the dimensions of the measuring apparatus, a rapid falling off in the viscosity coefficient takes place.

Knudsen (Ann. d. Phys. 28. 76. 1909) on the basis of the Kinetic Theory has deduced an expression for the rate at which gases at extremely low pressures flow through tubes, the free path being great compared with the tube diameters.

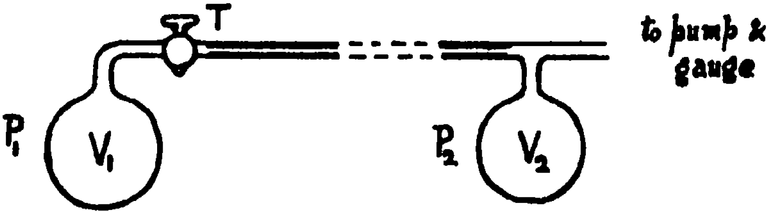


FIG. 1.

The two laws of gas flow through tubes—at ordinary pressures and at extremely low pressures—are very different. Neither law expresses the facts in the intermediate pressure region. It should be possible to deduce from the Kinetic Theory an expression for the flow to cover the whole range of pressure.

The experiments described in this paper were carried out with the object of finding an empirical expression for the flow of a gas through a tube which would represent the facts for all pressures.

The principal part of the apparatus is shown diagrammatically in Fig. 1. V_1 was a glass vessel of 164 c.c. capacity up to the wide bore tap T. V_2 was another glass vessel connected to a Toepler pump and a McLeod gauge. A capillary tube of known dimensions connected the two vessels. Since the smallest leak would render the results useless the connections throughout were of glass. Tap T having been open long enough for equilibrium, the pressure P_1 throughout the apparatus was read on the gauge. Then the tap was closed and the pressure in V_2 reduced to a value A. Next communication was made for a definite time t, measured with a stop-watch, the pressure in V_2 rising to a value B. Finally the tap was opened again and left open until the equilibrium pressure P was reached.

We have $P(V_1 + V_2) = P_1V_1 + P_2V_2$ . . . . (1)

If the volume flowing per second from V_1 is v_1, and the volume flowing per second into V_2 is v_2, then

$$P_1v_1 = P_2v_2 = \frac{P_1^2 - P_2^2}{16 \cdot l \cdot \eta} \pi a^4 \quad . \qquad . \; . \; . \quad (2)$$

where l = length of capillary

a = radius „ „

η = viscosity coefficient.

Also

$$\left.\begin{aligned} P_1v_1 &= V_2 \frac{dP_2}{dt} \\ P_2v_2 &= V_1 \frac{dP_1}{dt} \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3)$$

so that

$$dt = V_2 \frac{dP_2}{v_2P_2}$$

or

$$t = V \int_{P_2=A}^{P_2=B} \frac{dP_2}{v_2P_2} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4)$$

Substituting from (2) gives

$$t = V_2 \int \frac{dP_2}{\frac{P_1^2 - P_2^2}{16 l \eta} \pi a^4}$$

But from (1)

$$P_1 = \frac{P(V_1 + V_2) - P_2V_2}{V_1}$$

therefore

$$t = \frac{16\,l\,\eta}{\pi a^4} V_2 \int_A^B \frac{dP_2}{\left\{\frac{P(V_1 + V_2) - P_2V_2}{V_1}\right\}^2 - P_2^2}$$

$$= \frac{8\,l\,\eta}{\pi a^4} \frac{V_2V_1}{P(V_2 + V_1)} \log_e \left\{ \frac{P - A}{P - B} \frac{P\frac{V_2 + V_1}{V_2 - V_1} - B}{P\frac{V_2 + V_1}{V_2 - V_1} - A} \right\} \quad . \quad (5)$$

where A = initial pressure
B = pressure after time t } in vessel V_2
P = final pressure

Since $V_2 = \frac{P_1 - P}{P - A} V_1$, where P_1 is initial pressure in V_1, we may write equation (5):—

$$t = \frac{8\,l\,\eta\,V_1}{P\pi a^4} \frac{P_1 - P}{P_1 - A}$$

$$\log_e \left\{ \frac{(P - A)}{(P - B)} \frac{P(P_1 - A) - B(P_1 + A - 2P)}{P(P_1 - A) - A(P_1 + A - 2P)} \right\} \quad . \quad . \quad (5a)$$

whence we get η from the experimental data.

The volume V_1 was constant throughout the experiments and equal to 164 c.c. In most of the experiments V_2 was slightly less than V_1 and varied with the height of the barometer since it was in communication with the Toepler pump.

The mean pressure during a flow can be shown to be

$$\tfrac{1}{2}(P_1 + A) - \tfrac{1}{4}\{P_1 - a(A - B) + B\}$$

where $a = V_2/V_1 = (P_1 - P)/(P - A)$.

No account was taken of temperature variations, all experiments being carried out at room temperature.

The results are given in the following tables:—

AIR

Length of tube = 5 53 cm Radius of bore = 0 0186 cm

P_1 cm	A cm	B cm	P cm	t sec	Mean Press	η
0 592	0 048	0 088	0 330	40	0 321	0 000120
0 330	0 032	0 073	0 185	120	0 181	0 000101
0 185	0 022	0 055	0 106	240	0 104	0 0000737
0 111	0 0175	0 040	0 067	360	0 065	0 0000561
0 0670	0 0140	0 026	0 041	360	0 041	0 0000408
0 0415	0 0031	0 015	0 023	600	0 024	0 0000212
0 023	0 0046	0 0098	0 0136	600	0 0137	0 0000125
0 0136	0 0025	0 0057	0 0082	600	0 0081	0 00000840
0 0082	0 0016	0 00345	0 0051	600	0 0049	0 00000548
0 0051	0 00075	0 0021	0 0030	600	0 0030	0 00000314
0 0030	0 00050	0 00123	0 00180	600	0 00176	0 00000187

Length of tube = 57 0 cm Radius of bore = 0 0648 cm

P cm.	A cm	B cm	P cm	t sec	Mean Press	η
0 214	0 016	0·082	0 118	60	0 116	0 000138
0 118	0 019	0 046	0 070	60	0 068	0 000113
0 073	0 0145	0 0305	0 0445	80	0 044	0 0000952
0 0445	0 0060	0 0150	0 0255	80	0 0253	0 0000662
0 0225	0 0040	0 0085	0 0150	80	0 0136	0 0000462
0 0154	0 00295	0 00580	0 0096	100	0 0093	0 0000367
0 0096	0 0013	0 0031	0 0057	100	0 0055	0 0000234
1 330	0 335	0 655	0 845	10	0 836	0 000174
0 845	0 075	0 295	0 460	15	0 460	0 000160

Length of tube = 43 7 cm Radius of bore = 0 1065 cm

P_1 cm	A cm	B cm	P cm	t sec	Mean Press	η
0 105	0 123	0 039	0 065	5	0 064	0 000134
0 0395	0 0070	0 0160	0 0235	15	0 0233	0 0000841
0 0235	0 0030	0 0077	0 0135	15	0 033	0 0000651
0 0136	0 0026	0 00475	0 0085	15	0 0082	0 0000560
0 0085	0 0014	0 0029	0 0049	20	0 0049	0 0000322
0·0050	0 00070	0·00165	0·00290	20	0 00286	0 0000193

Length of tube = 3·72 cm. Radius of bore = 0·0101 cm.

P_1 cm.	A cm	B cm	P cm	t sec	Mean Press.	η
0·400	0·036	0·086	0·221	600	0·219	0·0000749
0·221	0·033	0·062	0·130	900	0·128	0·0000594
0·130	0·019	0·040	0·072	1,200	0·071	0·0000287
0·076	0·0165	0·030	0·046	1,800	0·045	0·0000236
0·046	0·0070	0·0160	0·027	1,860	0,027	0·0000112
0·0270	0·0024	0·0075	0·0145	1,800	0·014	0·0000081

Length of tube = 60·2 cm. Radius of bore = 0·0417 cm.

P_1 cm	A cm	B cm	P cm	t sec	Mean Press	η
0·720	0·127	0·297	0·425	100	0·424	0·000161
0·425	0·055	0·146	0·240	120	0·239	0·000135
0·242	0·027	0·073	0·136	150	0·135	0·000122
0·136	0·022	0·041	0·080	150	0·080	0·000098
0·082	0·018	0·028	0·051	180	0·051	0·0000651
0·051	0·0082	0·0142	0·0295	200	0·029	0·0000566
0·0295	0·0042	0·0079	0·0179	240	0·017	0·0000382
0·0170	0·0032	0·0056	0·0101	300	0·010	0·0000224

HYDROGEN.

Length of tube = 60·2 cm. Radius of bore = 0·0417 cm

P_1 cm	A cm.	B cm.	P cm	t sec		η
0·355	0·057	0·097	0·208	30	Mean press. practically P cms.	0·0000650
0·208	0·025	0·049	0·116	40		0·0000480
0·116	0·012	0·026	0·064	60		0·0000392
0·067	0·008	0·0175	0·0375	80		0·0000246
0·0380	0·0045	0·0105	0·0215	100		0·0000160
0·0215	0·0030	0·0060	0·0122	100		0·0000098
0·650	0·185	0·251	0·415	20		0·0000771
0·412	0·045	0·100	0·232	30		0·0000649
0·232	0·024	0·052	0·130	40		0·0000552
0·134	0·014	0·037	0·075	80		0·0000372
0·075	0·0093	0·022	0·042	100		0·0000271
0·042	0·0050	0·0130	0·0237	120		0·0000164
1·065	0·135	0·305	0·600	20		0·0000855
0·600	0·070	0·163	0·333	30		0·0000718
0·333	0·037	0·088	0·188	40		0·0000591
0·197	0·022	0·0515	0·109	60		0·0000499

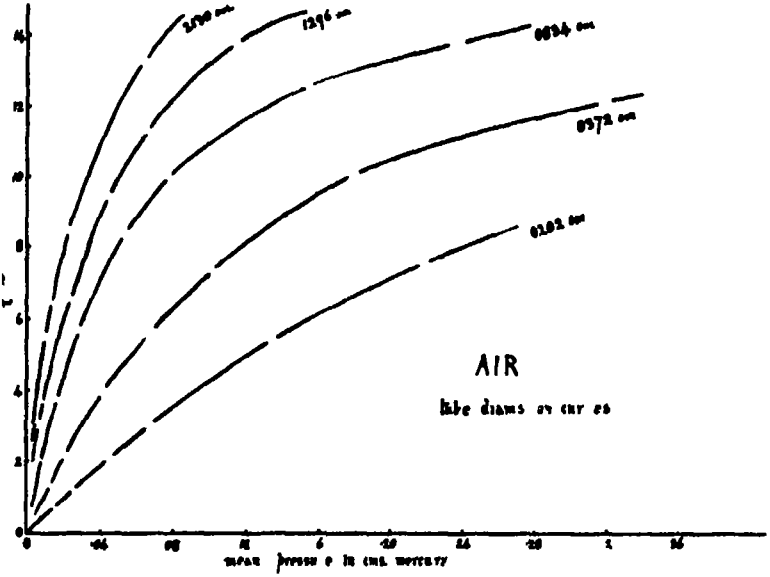

2590 cm
1296 cm
0834 cm
0572 cm
0282 cm
14
12
10
8
6
4
2
0
AIR
tube diams as curves
0
·04
·08
·12
·16
·20
·24
·28
·32
·36
mean pressure in cms mercury

FIG 2

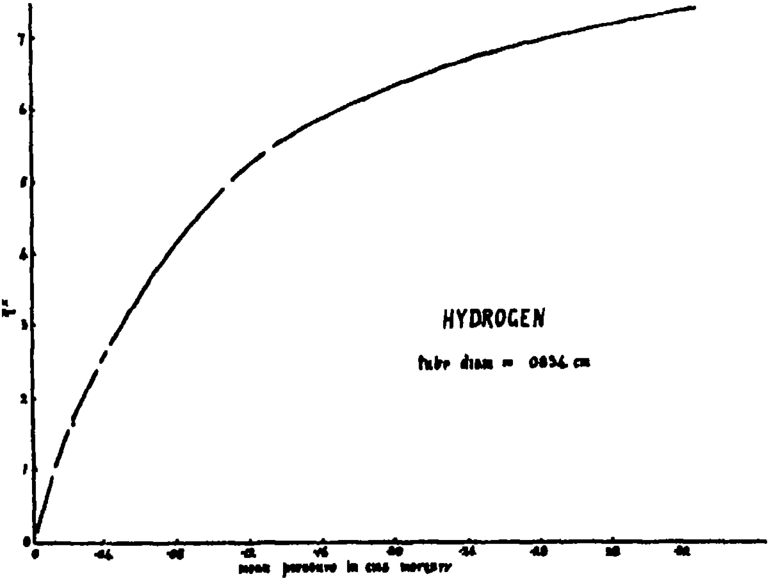

7
6
5
4
3
2
1
0
HYDROGEN
tube diam = 0836 cm
0
·04
·08
·12
·16
·20
·24
·28
·32
·36
mean pressure in cms mercury

FIG 3

The results in the above tables are shown graphically in Figs. 2 and 3. Examination of the curves for air (Fig 2) shows that the viscosity of air flowing along a tube of circular cross-section is a function of the product of mean pressure into the diameter, *i.e.*, $\eta = \phi(pd)$.

Evidence for this is given in the following table which has been taken from the curves in Fig. 2.

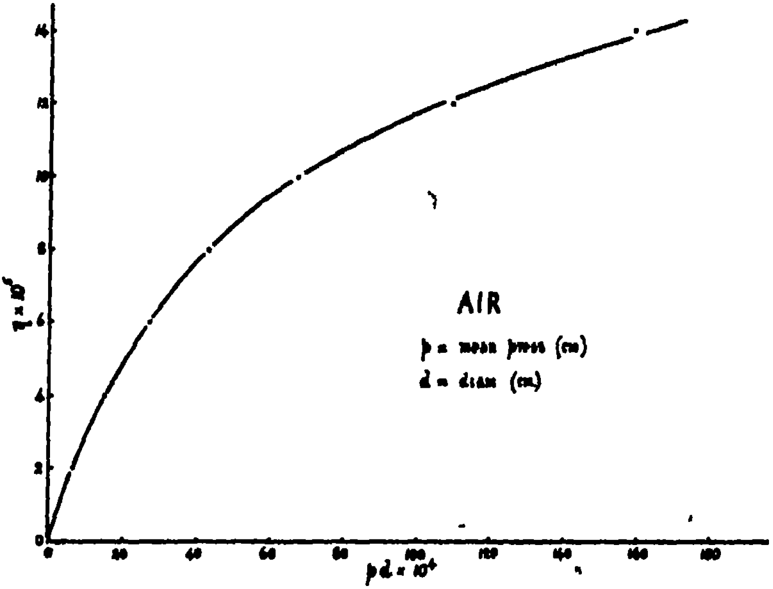


FIG. 4.

AIR.

$\eta \times 10^5$	$pd \times 10^4$ for tubes of diameter.—				
	·2130 cm	·1266 cm	·0834 cm	·0372 cm	·0202 cm.
2	6	7	7	7	8
4	13	14	15	16	18
6	23	27	26	28	30
8	43	43	41	43	47
10	68	64	66	67	70
12	103	100	110	115	
14	160	160			

Thus the relation between viscosity mean pressure and tube diameter can be represented by one continuous curve For air the curve is shown in Fig 4

As to the form of the function ϕ in the equation $\eta_p = \phi(pd)$ the curve in Fig 4 suggests the relation $\eta_p = \eta_\infty (1 - e^{kpd})$ where η_∞ is the viscosity at high pressures and k is a constant This equation only roughly fits the curve It can be made to fit more exactly by adding terms to the exponent of e At high pressures the expression becomes $\eta_p = \eta_\infty$ while at very low pressures it becomes $\eta_p = \eta_\infty\ k\,pd$

Let us compare this with Knudsen s formula

For extremely low pressures Knudsen gives for the flow along tubes the expression

$$-\frac{d}{dt}PV = \frac{d}{dt}PV_2 = 3809\sqrt{\frac{T}{M}}\,\frac{d^3}{l}(P_1 - P_2)$$

where P is in dynes T is absolutely temp M is molecular weight of the gas d is the tube diameter and l is the length of the tube It follows that

$$V_2\frac{dP_2}{dt} = c\frac{d^3}{l}(P - P_2) \qquad \text{where } c = \text{const}$$

therefore
$$V_2\int_{P_2=A}^{P_2=B}\frac{dP_2}{P - P} = c\frac{d^3}{l}t$$

which is the same as
$$\frac{V_1V}{V + V_2}\int_A^B\frac{dP_2}{P - P} = c\frac{d^3}{l}t$$

whence
$$t = \frac{l}{cd^3}\,\frac{V_2V}{V + V_2}\log\frac{P - A}{P - B}$$

Now equation (5) is
$$t = \frac{\eta l}{\pi a^4 P}\,\frac{V_2V_1}{V + V_2}\log_e\frac{P - A}{P \quad B}$$

(when V_1 and V_2 are equal P is mean pressure)

Since at very low pressure $\eta_p = \eta_\infty\ k\ P\ d$ we may write

$$t = \frac{\eta_\infty kl}{\pi d^3}\,\frac{V_2V_1}{V_1 + V_2}\log_e\frac{P - A}{P - B}$$

which agrees with Knudsen s expresson if $c = \frac{\pi}{k\eta_\infty}$

THE INFLUENCE OF ELECTRIC POTENTIAL UPON THE VELOCITY OF FERMENTATION.

By M. C. POTTER, Sc.D., M.A.

[Read 14th March, 1916.]

In a previous paper[1] it has been shown that a rise of potential amounting to as much as ·3 to ·5 volt is produced by yeast when growing in an ordinary fermentable liquid, and a correspondng E.M.F. is developed during the decomposition of organic matter by bacteria. One of the problems suggested during this investigation was the possible influence of this rise of potential upon the velocity of the reaction. Would fermentation or putrefaction proceed more rapidly, or the reverse, if the nutrient media were connected with the earth and thus maintained at zero potential, or would the velocity of these reactions be accelerated or retarded when they take place in insulated flasks? In other words would the vital activity of an organism be affected by the variations of the potential level of the medium in which it is living, in a manner corresponding to such stimuli as light or tempera-trueP

An investigation to decide this point has been carried out by a comparison of the rate of fermentation of glucose by yeast in a flask carefully insulated or raised to a definite potential, with that in a flask in which the glucose was earthed. It will be understood that two similar flasks were employed, each containing the same amount of the fermenting media and the same quantity of yeast, and that both were maintained under precisely similar external conditions; and it will be observed that only a differential result between the two flasks is required.

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[1] Proceedings of the University of Durham Philosophical Society, vol iii., 1910.

The problem of estimating the velocity of alcoholic fermentation has engaged the attention of many investigators, and various methods have been devised to measure this velocity. The method chiefly relied upon in this research has been the evolution of the CO_2, and the methods indicating the rise of temperature and the rate of the disappearance of the sugar have also been employed for the purpose of confirmatory tests.

THE EVOLUTION OF CO_2.

In estimating the velocity of fermentation by the amount of CO_2 produced in a given time, a modification of Slator's[1] method was adopted in the present investigation. In Slator's method the fermenting sugar is contained in a flask maintained at a constant temperature by means of a thermostat; from the flask an india rubber tube leads to a manometer. The velocity of fermentation is then measured by the amount of CO_2 evolved during a given time, as evidenced by the pressure registered by the manometer Before each reading is taken, the fermenting liquor is vigorously shaken by the hand, to liberate the entangled bubbles of gas.

In this research the apparatus was specially designed to avoid shaking by the hand. As the velocity of the fermentation is being measured by the CO_2 generated, and the determination of pressure is involved, it is exceedingly important that all parts of the apparatus employed should be maintained at a constant temperature. A Hearson's incubator was employed, working at a temperature of 23°C. Inside the incubator a wooden frame was fitted, the top bar carrying an iron rod from which the two flasks containing the fermenting liquid could be suspended. The iron rod was sufficiently long to project through a detachable wooden door, fitted to the incubator, and to this end a crank was fastened. This crank was joined by means of a connecting rod to a crank driven by an electric motor.

In this way the fermenting liquid could be evenly and vigorously shaken, while being maintained at a constant temperature inside the incubator, and the risk of error in the manometer readings through handling the flasks was thus avoided.

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[1] Journal of the Chemical Society, vol. lxxxix., 1906.

The mouth of the flasks containing the fermenting liquid was closed by a rubber stopper perforated by two holes, one for the introduction of an electrode and the other for the insertion of a glass tube. The electrode was formed of a short length of platinum wire soldered to a copper wire and fused into a glass tube as described in a previous paper.[1] The glass tube was connected by a thick-walled rubber tube, passing through a perforation in the detachable door, to the manometer; the manometer being attached to the outside of the detachable door of the incubator. The whole of the apparatus could be readily removed and replaced in the incubator.

It will be clear from the above description that up to this stage the conditions are the same for the two flasks. It now remains to show how one could be insulated, while the other could be earthed or raised to a definite potential. For the purpose of insulation, ebonite plates were fitted round the necks of one of the flasks before it was clamped in position, and in the length of rubber tubing connecting this flask to the manometer a short length of ebonite tube was inserted. To test the insulation an electrode from this flask was connected to a gold leaf electroscope. The electroscope was found to remain charged for some hours, showing that the leakage of electricity could be neglected.

To raise the other flask to a definite potential or to connect it to earth, a short glass tube sufficiently long to pass through the detachable door of the incubator was firmly fixed to the wooden frame. Through the centre of this glass tube a copper wire was fixed by means of paraffin. The end of the wire projecting into the interior of the incubator was attached by means of a coiled brass wire to the electrode of the flask while the exterior end could be connected to a battery or to earth.

The general method of procedure was to set up the wooden frame and flasks, and to place it, together with a flask containing the glucose solution, inside the incubator for a sufficient length of time to allow the whole apparatus to assume the temperature of the incubator. When starting the experiment a weighed amount of commercial yeast was added to a definite quantity of the glucose solution and stirred until the yeast-cells were all separated from each other. By means of a pipette an equal quantity of this yeast-glucose was placed in each flask, and also an equal quantity of glucose solution. Generally the proportion for the yeast-glucose was 10 grams of yeast in one 100 c.c. of a 10 per cent. glucose solution. As quickly as possible the necessary connections were then made, the motor and crank connection set in motion and the flasks shaken within the incubator.

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[1] Proceedings of the Royal Society B., vol. lxxxiv., 1911.

The rubber tubes from the flasks were connected with the manometers by means of a three-way tap, so that the pressure could be reduced to that of the atmosphere after each reading.

As a typical example of several experiments 180 c.c. of a 10 per cent. solution of glucose was placed in each flask, and to this was added 20 c.c. of the same solution containing 20 grams of yeast. Thus each flask contained 200 c.c. of a 10 per cent. solution of glucose, together with 20 grams of yeast.

In Table I. is given the readings from a typical experiment. Column A gives the times at which the readings were taken, namely every five minutes during two and a quarter hours. Column B gives the pressure developed in centimetres of mercury during each five minutes. It is seen that during the first ten minutes the pressure is higher than during the succeeding intervals, but that after that time the pressure developed during the succeeding intervals is uniform within the limits of experimental error. Column C gives the pressures (1) when the flask was earthed; these agree with the corresponding readings in column B, except that the last readings are one millimetre below those in column B, (2) when the fermenting liquid was raised to 210 volts positive; the readings are throughout one milimetre less than the corresponding readings in column B, and hence it may be inferred that raising the voltage to 210 volts positive does not effect the rate of fermentation, (3) when raised to a voltage of 210 volts negative; the figures in B and C again agree, and thus when raised to this voltage the rate of fermentation remains unaltered. A continuation of the experiment merely confirmed the previous results.

TABLE I.

A.	B.	C.	
10·26			Earthed
10·31	1·2	1·2	
10·36	1·4	1·4	
10·41	0·9	0·9	
10·46	1	0·9	
10·51	1	0·9	
10·56	1	0·9	
11·1	1	0·9	
11·6	1·1	1	210 volts + ve
11·11	1	1	
11·16	1·2	1·1	
11·21	1·2	1·1	
11·26	1·2	1·1	
11·31	1·2	1·1	210 volts − ve
11·36	1·2	1·1	
11·41	1·3	1·3	
11·46	1·2	1·1	
11·51	1·2	1·1	
11·56	1	1·1	
12·1	1·1	1·1	210 volts + ve
12·6	1·2	1·1	
12·11	1·2	1·1	
12·16	1·1	1·1	
12·21	1·1	1	
12·26	1·1	1·1	210 volts ve
12·31	1·2	1·1	
12·36	1·1	1·1	
12·41	1·1	1·1	
12·46	1·1	0·9	

TABLE 1.—In column A is given the times at which the observations were taken. In B, the control flask, the pressure developed in each succeeding five minutes In C, the second flask, the pressure developed during the same intervals of time when this flask was earthed or raised to a potential of 210 volts, alternately positive and negative The pressures are given in centimetres of mercury

RISE OF TEMPERATURE.

In estimating the velocity of fermentation by means of the rise in temperature, special precautions are necessary to guard against any varying conditions which might give rise to fluctuations of temperature. In these experiments silvered Dewar's flasks were employed. They were further protected by being placed side by side in an Hearson's incubator or suitable box for several hours, so that they should assume the same temperature.

The general procedure adopted was to prepare a sufficient quantity of a 10 or 15 per cent. solution of glucose, to add the yeast previously disintegrated in water, and then to thoroughly mix by repeated pouring from one vessel to another. Equal quantities of this mixture were then poured into the Dewar's flasks, which were insulated by ebonite or paraffin; one of these was earthed by means of a platinum electrode, while the other by a similar electrode could be earthed or raised to a known potential. Any difference in temperature was measured by means of standardised mercurial thermometers, and also by a thermopile and galvanometer. Within the limits of experimental error the rise of temperature in these flasks was the same, and no clear indication could be obtained that the fermentation proceeded more rapidly in the flask maintained at zero potential than in the one raised to a definite potential.

RATE OF DISAPPEARANCE OF THE GLUCOSE.

Using all the precautions as in the experiments just described, the rate of the disappearance of the glucose was determined by means of a Schmidt and Haensch polarimeter. Measured in this way the rate of fermentation was the same in the earthed flask and the one raised to a definite potential.

All these experiments were repeated on several occasions, and were found to be in close agreement, and it may be inferred that the low voltages, 210 volt., such as that employed does not influence the velocity of fermentation.

ON A CHARACTERISTIC PROPERTY OF THE TIDAL STREAMS OF THE STRAIT OF DOVER.

By G R Goldsbrough, D Sc, F R A S

§ 1. As a result of the exhaustive observations of the tidal currents of the English Channel by Admiral Beechy,[1] it is shown that the Channel can be divided up into separate areas according to the character of the motions of the tidal currents therein:

A. The area bounded by lines joining the Start, Casquets, point d'Ailly and Beachy Head. "In this area the whole body of the water moves eastward towards the North Sea whilst the tide is rising at Dover and westward when it is falling there."[2]

B. The area bounded by lines joining the North Foreland, the Leman and Owen lightship, the Hook of Holland and Dunkerque. "In this area the whole body of water moves south-westward towards the English Channel when the tide is rising at Dover and eastward when it is falling there."[2]

Between these two areas there is the Strait of Dover. The effect of the oscillating area on each side is to produce a more complicated motion in the Strait. It is observed that there is a definite line of "junction of the tides" or "separation of the tides" which moves from the western to the eastern boundary. At this line there is no current, and so definite is it that Beechy reports that two ships riding at anchor one mile apart were found to have their heads in opposite directions. This line is found to take the following positions:[3]

(1) Line joining Beachy Head and point d'Ailly—5 hours before and 1 hour after high water at Dover.

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[1] *Phil. Trans*, vol 141, p 703

[2] *Tides and Tidal Streams of the British Islands*, pp 12, 13

[3] *Ibid* p 35

(2) Line joining Hastings and Tréport—4 hours before and 2 hours after high water at Dover.
(3) Line joining Dungeness and Quentin—3 hours before and 3 hours after high water at Dover.
(4) Line joining Folkestone and Boulogne—2 hours before and 4 hours after high water at Dover.
(5) Line joining South Foreland and Calais—1 hour before and 5 hours after high water at Dover.
(6) Line joining North Foreland and Dunkerque—high water at Dover and 6 hours after high water there.

These observations give the time in round figures. It is clear that the recurrence should be, for the semi-diurnal tide, after 6 hours $12\frac{1}{2}$ minutes and not 6 hours exactly.

Beechy's observations also show that there is practically no transverse motion of the water in the Channel: it may be taken as wholly longitudinal.

§ 2. We have therefore to explain first, the existence of such a definite no-current line; and second, its rate of movement. The explanation generally offered is that the definiteness is due to the constriction of the Channel at Dover. The following analysis proves that this explanation is correct.

Consider the long waves formed in a canal of slowly varying section. Take the axis of x along the direction of the canal and let $A(x)$ be the area of cross-section, and $b(x)$ the breadth of the surface, both taken normal to the x-axis, at the point x. If η is the height of the surface above the undisturbed level, and ξ is the horizontal displacement of the particles in the vertical plane through x, both at time t, then[1]

$$\eta = -\frac{1}{b(x)}\frac{\delta}{\delta x}\left\{ A(x)\,\xi \right\} \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

$$\frac{\delta^2 \xi}{\delta t^2} = -g\frac{\delta \eta}{\delta x} \quad . \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

On eliminating η between (1) and (2),

$$\frac{\delta^2}{\delta t^2}\left\{ A(x)\xi \right\} = gA(x)\frac{\delta}{\delta x}\left[\frac{1}{b(x)}\frac{\delta}{\delta x}\left\{ A(x)\xi \right\}\right] \quad . \quad . \qquad (3)$$

---

[1] *Lamb Hydrodynamics*, Fourth Ed, p 247.

If the canal has a constant mean depth h, then $A(x) = h\, b(x)$. We shall assume that the Strait of Dover corresponds roughly to a curve defined by $b(x) = b \sec^2 x/a$, b and a being constants. Then, on putting $u = \frac{\delta\xi}{\delta t}$, and $A(x)\, u = v$, we have

$$\frac{\delta^2 v}{\delta t^2} = gh \sec^2\frac{x}{a}\frac{\delta}{\delta x}\left[\frac{1}{\sec^2 x/a}\frac{\delta v}{\delta x}\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The canal we are considering connects two independently tided seas. We shall suppose the oscillations of these seas can be expressed in the form

$$u = V \sin \sigma t + U \cos \sigma t, \text{ where } \sigma = 2\pi \div 12 \text{ hours } 25 \text{ minutes.}$$

Assuming then that we have simple harmonic vibrations in the canal, we find, as the solution of (4)

$$v = \sec x/a\,[\{A \sin \kappa x/a + B \cos \kappa x/a\}\sin \sigma t$$
$$+ \{C \sin \kappa x/a + D \cos \kappa x/a\}\cos \sigma t];$$

or

$$u = \cos x/a\,[\{A \sin \kappa x/a + B \cos \kappa x/a\}\sin \sigma t$$
$$+ \{C \sin \kappa x/a + D \cos \kappa x/a\}\cos \sigma t], \quad . \quad (5)$$

where $\kappa^2 = \sigma^2 a^2/gh + 1$, A, B, C and D being arbitrary constants.

In the application of these results to the tidal phenomena of the Strait of Dover, it will be necessary, in order to give a closer representation, to choose portions of the two curves $b(x) = b \sec^2 x/a$ and $b'(x) = b \sec^2 x/a'$. The origin will be taken at the narrowest part, where the breadth is b; the first curve will represent the easterly portion of the Strait and the second, the westerly.

The current in each part will be determined by an equation of the form (5). To this must be added the condition that at the origin η and u must be alike for both portions.

From (5) and (1) we find

$$\eta = \frac{\cos \sigma t}{\sigma a b}\{\sin x/a\,(A \sin \kappa x/a + B \cos \kappa x/a)$$
$$+ \kappa \cos x/a\,(A \cos \kappa x/a - B \sin \kappa x/a)\}$$
$$- \frac{\sin \sigma t}{\sigma a b}\{\sin x/a\,(C \sin \kappa x/a + D \cos \kappa x/a)$$
$$+ \kappa \cos x/a\,(C \cos \kappa x/a - D \sin \kappa x/a)\} \quad . \quad (6)$$

Suppose that unaccented letters refer to the positive part of the canal and accented letters refer to the negative part. Then the conditions at the origin are

$$\mathrm{B} = \mathrm{B}', \ \mathrm{D} = \mathrm{D},$$
$$\frac{\kappa \mathrm{A}}{a} = \frac{\kappa' \mathrm{A}'}{a}, \ \frac{\kappa \mathrm{C}}{a} = \frac{\kappa' \mathrm{C}'}{a'} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \qquad (7)$$

Also, let the junctions with the seas be at $x = l$ and $x = -l'$. Then, when $x = l$, $u = \mathrm{V} \sin \sigma t + \mathrm{U} \cos \sigma t$, and when

$$x = -l', \ u = \mathrm{V}' \sin \sigma t + \mathrm{U}' \cos \sigma t.$$

Hence the further equations of condition are:

$$\left.\begin{aligned} &\cos l/a \{\mathrm{A} \sin \kappa l/a + \mathrm{B} \cos \kappa l/a\} = \mathrm{V}, \\ &\cos l/a \{\mathrm{C} \sin \kappa l/a + \mathrm{D} \cos \kappa l/a\} = \mathrm{U}, \\ &\cos l'/a' \{-\mathrm{A}' \sin \kappa' l'/a' + \mathrm{B}' \cos \kappa' l'/a'\} = \mathrm{V}', \\ &\cos l'/a' \{-\mathrm{C} \sin \kappa' l'/a' + \mathrm{D} \cos \kappa' l'/a'\} = \mathrm{U}'. \end{aligned}\right\} \quad . \qquad (7)$$

These can be directly solved for A, B, C, D, A′, B′, C′, D′ in terms of V, U, V′, U . We find then:

$$\left.\begin{aligned} &\mathrm{A} = a\kappa \{\mathrm{V} \sec l/a \cos \kappa\, l'a' - \mathrm{V}' \sec l/a \cos \kappa l/a\} - d, \\ &\mathrm{A}' = a'\kappa \mathrm{A} - a\kappa, \\ &\mathrm{B}' = \mathrm{B} = \{a\kappa' \mathrm{V}' \sec l'/a' \sin \kappa l/a + a\kappa \mathrm{V} \sec l/a \sin \kappa' l'/a'\} - d, \\ &\mathrm{C} = a\kappa' \{\mathrm{U} \sec l/a \cos \kappa' l'/a' - \mathrm{U}' \sec l'/a' \cos \kappa l/a\} \div d, \\ &\mathrm{C}' = \mathrm{C} a\, \kappa - a\kappa', \\ &\mathrm{D}' = \mathrm{D} = \{a\kappa' \mathrm{U}' \sec l'/a \ \sin \kappa l/a + a'\kappa \mathrm{U} \sec l,a \sin \kappa' l'a'\} - d, \\ &d = a\kappa' \sin \kappa l/a \cos \kappa' l'/a' + a'\kappa \sin \kappa' l'/a' \cos \kappa l/a. \end{aligned}\right\} \ (8)$$

The vanishing of d can readily be shown to be the condition of resonance.

§ 3. The travelling line of no-current exists as a common property of all long waves in one dimension. For example, in the case of a single wave propagated in a straight canal of uniform depth and breadth, we may take

$$u = \mathrm{P} \sin (\sigma t - kx), \text{ where } \sigma k = \surd(gh) \quad . \ . \quad . \qquad (9)$$

The current is zero along the line $kx = \sigma t$, which moves with the speed of the wave. In general, however, this is only recognisable as a period of "slack water," which is vaguely bounded both in time and space. Its definiteness depends upon the magnitude of $\delta u/\delta x$, greater values of this derivative giving greater precision to the position of the line. In the case of (9) $\delta u/\delta x = -kP$, when $kx = \sigma t$. We may compare this with a simple wave travelling along the canal of variable breadth dealt with in § 2. This wave may be taken as

$$u = P' \cos x/a \sin(\sigma t - \kappa x/a) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In this case $\delta u/\delta x = -P'\kappa/a \cdot \cos x/a$, ... (11)

$$\text{when } \sigma t = \kappa x/a.$$

In both cases the arbitrary constants are determined by the given value of u at some distant point. Hence P and $P' \cos x/a$, where x is large but less than $\pi a/2$, will be of approximately the same value. So that P' will be greater than P. And $\delta u/\delta x$ for the second case will be the greater especially when the line of no-current is not far from the origin. Equation (11) also shows that as x increases the line diminishes in definiteness.

It is clear then, that if the Strait of Dover can be approximately represented by secant curves in the manner indicated, the pronounced no-current line will follow. It remains to examine the point numerically, to see if approximate agreement with observation results. There are, however, one or two inteaesting properties of the wave motion, as given by (5) and (6) worth mentioning.

§ 4. From equation (5) the no-current line is given by

$$\cos x/a = 0, \text{ or}$$

$$\tan \sigma t = -\{C \tan \kappa x/a + D\} \div \{A \tan \kappa x/a + B\}. \quad (12)$$

The first equation must be discarded as it is satisfied first by $x/a = \pi/2$, which would imply that the canal extended to infinite breadth. The second equation may be written alternatively,

$$\tan \kappa x/a = -(B \tan \sigma t + D) \div (A \tan \sigma t + C) \quad . \quad (13)$$

It is readily shown from either (12) or (13) that for every value of x there is a corresponding value of t and *vice-versâ*. Hence there is no part of the canal in which the no-current line does not appear at some time. Also, if x_1 is a solution of (13) corresponding to a value t_1 of t, then also $x_1 + n\pi a/\kappa$ is a solution, where n is any integer. Thus the no-current lines follow each other at a distance $\pi a/\kappa$, or half a wave length. Similarly if a certain point is on the no-current line at time t_1, it will again be on the no-current line at time $t_1 + r\pi/\sigma$ or, after a multiple of the half-period.

The velocity of a no-current line is

$$-\frac{a\sigma}{\kappa}\frac{(BC-AD)\sec^2\sigma t}{(A\tan\sigma t+C)^2+(B\tan\sigma t+D)^2} \quad . \quad . \quad . \qquad (14)$$

The sign of this quantity depends upon the constants A, B, C, D, and finally on the sign of (BC−AD), the remaining factors being positive for all values of t. In other words, the direction of motion of the no-current line is fixed by the constants of motion of the connected seas. We assumed before that at the limits of the canal,

$$u = V\sin\sigma t + U\cos\sigma t,$$

$$\text{and } u' = V'\sin\sigma t + U'\cos\sigma t.$$

If these vibrations are in the same phase, by a suitable change of the origin of the time, we can make either $V=V'=0$, or $U=U'=0$. Reference to (8) shows that in the first case, $A=B=0$, and in the second, $C=D=0$. In either event the value of $BC-AD$ is zero. Again, if the boundary vibrations are in exactly opposite phase, it could be arranged similarly that either $V=V'=0$, or $U=U'=0$. So that a moving no-current line only appears when a phase difference other than a half period exists. In the zero case (13) shows that $\tan\kappa x/a = -B/A$ or $-D/C$. We have then in the canal a standing oscillation.

The velocity at a short distance δx from a no-current line is given by

$$u = \kappa/a\,\delta x\cos x/a\sec\kappa x/a\cos\sigma t(BC-AD)\div(A\tan\kappa x/a+B) \qquad (15)$$

In this formula x and t are related by (12) or (13).

If x and t are fixed in value, u changes sign with δx. Hence the currents approach the zero line from both sides or recede on both sides, as might otherwise have been shown. This exhibits the nautical statement of the "meeting" and "separating" of the tidal currents.

As has already been shown, the no-current line will be at a point x at time t and again at time $t+\pi/\sigma$. Hence, owing to the presence of the factor cos σt in (15), if u has a certain sign and value at a distance δx from the no-current line at a point x and time t, it will have the opposite sign at the same place for the next return of the no-current line which occurs at time $t+\pi\sigma$. That is, if the currents approach the no-current line as it passes through a given point, they will recede from the next no-current line which passes that point and *vice-versâ*. This is in agreement with the observations quoted in § 1. At the first stated time the tidal streams are said to " meet," at the second time they " separate."[1]

APPLICATION TO THE PHENOMENA OF THE STRAIT OF DOVER.

§ 5. In applying the preceding results to the tidal movements of the Strait of Dover, one must determine the constants of the problem from the motions at the boundaries of the area. This is a trifle difficult to do. The Admiralty manual gives a large number of observations at various places, but it is difficult to decide whether these are purely local or whether they may be regarded as holding over a larger area. A fair average value, however, seems to be 3½ knots for mean spring tides in the area from the Start to Beachy Head. For the south-western portion of the North Sea near to the Strait of Dover, we may take 2½ knots. It, however, the statement of Beechy that the current flows toward Dover when the tide is rising there, and conversely, be applied rigorously to the areas named A and B in § 1, it is clear from § 4, that the progressive no-current line could not result, as the phase-difference would be exactly 180°. We shall in place use the facts that the current is zero on the Beachy Head—point d'Ailly line 5 hours before high water at Dover, and also zero on the North Foreland—Dunkerque line at the time of high water at Dover. We have then sufficient information to determine the motion.

To determine the equation of breadth of the Strait of

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[1] *Tides and Tidal Streams*, p. 35.

Dover in the approximate form $b\ (x) = b \sec^2 x/a$ we have the following data —

Section	Width of Channel Statute Miles	Distance between Sections Statute Miles	Mean Depth across section Fathoms
Beachy Head—Point d Ailly	66 4	21 6	17 5
Hastings—Tréport	63 6	16 4	11 6
Dungeness—Quentin	42 0	16 4	13 3
Folkestone—Boulogne	30 6	15 9	11 0
South Foreland—Calais	23 0	13 1	12 3
North Foreland—Dunkerque	43 2		12 8
Mean			13 1

The table gives the distances from coast to coast at each of six sections being those of the observations in § 1 The second column gives the distances apart of the sections measured along mid channel The third column shows the mean depth across each section It will be noticed that excepting for the first section the depth is fairly constant On this account and because of the simplification of the analysis I have taken the value 13 1 fathoms or 79 feet as the value throughout

It is readily seen that the channel does not follow the assumed law of breadth $b(x) = b \sec^2 a/x$ with any close accuracy That law however is sufficiently exact to give results comparable in accuracy with the observations

Taking the origin of co-ordinates at the narrowest section we have then the two equations

(*a*) For the eastward portion $b\ (x) = 23 \sec^2 x/17\ 3$

and (*b*) For the westward portion $b\ (x) = 23 \sec^2 x/74\ 6$

From these we have

$\kappa = 1\ 032$ radians $= 59^\circ\ 8$

$\kappa = 1\ 489$ radians $= 85\ 19$

$A = 1\ 79\quad B = 3\ 06\quad C = -1\ 09,\ D = 1\ 06$

$A = 5\ 37\quad B = 3\ 06\quad C = -3\ 27\quad D = 1\ 06$

Using these values of the constants, we can now calculate the time of arrival of the no-current line at each of the positions quoted in the table and compare with the observations in § 1. The results are given in the following table:—

Position of No Current Line	Corresponding time by above theory, reckoned from time of high water at Dover	Corresponding time by observation
Beachy Head—Point d'Ailly	−5·2 hours. +1·0 "	−5 hours. +1 "
Hastings—Tréport	−4·46 " +1·74 "	−4 " +2 "
Dungeness—Quentin	−3·6 " +2·6 "	−3 " +3 "
Folkestone—Boulogne	−2·02 " +4·18 "	−2 " +4 "
South Foreland—Calais	−0·71 " +5·49 "	−1 " +5 "
North Foreland—Dunkerque	0 " +6·2 "	0 " 6 "

If we bear in mind that the observations are approximate only as is shown in the fact that the semi-period is quoted as 6 hours in place of 6·2 hours, it will be seen that the agreement between theory and observation is fairly good. A stricter representation of the shape of the strait would perhaps have improved the theory especially near the narrowest part. But the elaboration of the work would hardly seem to be justified in view of the difficulty of making more exact observations of the no-current line.

University of Durham Philosophical Society, 1914–19.

Revenue Account.

	£	s.	d.						£	s.	d.
To Subscriptions	48	0	0	By Stationery and Circulars					2	9	6
„ Sale of Reprints	2	1	6	„ Clerical Work and Postages					14	19	2
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Interest on Investments	27	0	8	„ Printing Proceedings :—							
				Vol. V., Part 4	£18	19	0				
				Vol. V., Part 5	10	6	0				
									29	5	0
				„ Bank Charges						17	4
				„ Reserve Fund					1	9	0
				„ Balance being Surplus for period 5th October, 1914, to 21st November, 1919					44	3	8
	£97	2	2						£97	2	2

Life Composition Fund.

	£	s.	d.		£	s.	d.
To Balance at 5th October, 1914	35	0	0	By Balance at 21st November, 1919	35	0	0

Reserve Fund.

	£	s.	d.		£	s.	d.
To Balance at 5th October, 1914	50	13	8	By Balance at 21st November, 1919	57	0	0
„ Interest to 3rd March, 1917	4	17	4				
„ Transfer from Revenue Account	1	9	0				
	£57	0	0		£57	0	0

Balance Sheet.

LIABILITIES.				£	s.	d.	ASSETS.	£	s.	d.
To Life Composition Fund				35	0	0	By 100—4½% War Loan (B.E.)	95	0	0
„ Reserve Fund				57	0	0	„ 30—5% „ „ (P.O.)	28	6	9
„ Revenue Account :—							„ 20—5% War Bond, 1927	20	0	0
Balance at 5th October, 1914	41	4	4				„ Newcastle Savings Bank	32	17	0
Surplus for period 5th October, 1914 to 21st November, 1919 .. .	44	3	8				„ Cash in Hand..	1	4	3
				85	8	0				
				£177	8	0		£177	8	0

November 21st, 1919. Audited and certified correct, R. N. Wilkinson. *Hon. Auditor.*

Vol. VI. Part 2

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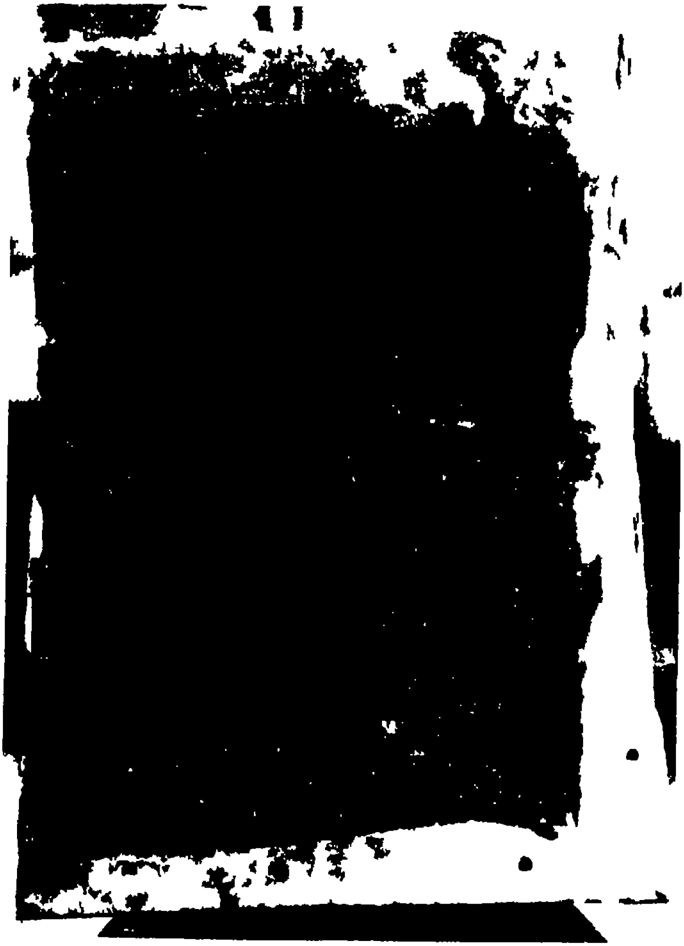

BRONZE TABLET FROM ALJUSTREL

University of Durham Philosophical Society

SOME ASPECTS OF MINING LAWS UNDER THE ROMAN EMPIRE

By HENRY LOUIS, M A , D Sc

[Read February 17th 1921]

The fact that Roman Jurists paid particular attention to the need of legislation in respect of mines, the ownership thereof and the relation of the State to mines and miners is very generally admitted and there is evidence that they must have done this at a very early period, the basis of a good deal of their practice was no doubt derived from Greek sources, mining in Greece going back to a very remote period There is comparatively little to be found on the subject of mining legislation in such of the earlier writings of the Roman Jurists as have come down to us, the Civil Code of Justinian (A D 528) devotes a short chapter to the subject, but even in this it is obvious that ancient customs and unwritten law played a very important part and that this code does little more than set forth definitely practices that probably were in existence from time immemorial A great deal of light has been thrown upon this subject by the discovery in Portugal of two Bronze Tablets found in 1876 and 1906 respectively, which, though they have been carefully studied on the Continent, appear to be practically unknown in this country It is to these that I wish to attract your attention to day and I need hardly say that in so doing I make no pretence at any originality, and that I am availing myself to the full of the translations, interpretations and comments of the Continental Archæologists to whom I have referred

Both these tablets were found buried in piles of old Roman slags in the mining district of Aljustrel, a small village in Southern Portugal, where copper mining has been carried on at irregular intervals from Roman times until quite recently. The ore bodies last worked consist of cupriferous iron pyrites now relatively poor in copper, carrying barely 2 per cent. of that metal. There is no doubt that the upper portions of the deposits were much richer in copper and they also probably carried a considerable proportion of silver. This phenomenon of the secondary enrichment of such ore deposits in their more superficial portions is quite familiar to mining geologists, especially in ore bodies of this type. It is quite clear from the numerous remains of workings, instruments, etc., that have been found here, that the Romans carried on active mining operations in this district, to which they gave the name of *Vicus Vipascensis*, and that they smelted on the spot the ores extracted from the mines, as is proved by the large piles of Roman slags still to be seen there. There is also some evidence that ore was brought from other mines into this mining district to be smelted, and it is probable that the Romans had here a metallurgical station for the smelting and refining of ores and metals.

The first of these tablets, found in 1876, is numbered III and is evidently the third of a series, though unfortunately the only one that has been discovered. It is divided into nine clauses (the last of which is incomplete), and refers exclusively to the financial administration of the mining camp, which was, as usual, subject to a "*Procurator Metallorum*," the representative of the Central Roman Authority, appointed probably from Rome, whose powers and duties appear to have been just about the same as those of the Cornish Warden of the Stannaries, or a Goldfield's Warden in an Australian Gold Mining Camp. It may be noted that the word Metallum had become by this time very sharply defined; in the same way as the Romans derived much of their mining knowledge and the basis of their mining legislation from the Greeks, so had they also adopted many Greek mining terms. The Greek word *μεταλλον* appears to have meant originally the actual mine opening. If the generally accepted derivation from *μεταλλάω* (I search after), is correct, it would correspond with what we should now call a prospect Of course in primitive times the man who discovered ore would himself extract it and would smelt it into metal on the spot so that the word μεταλλον was first applied both to the working and to its product Here however we find the word used quite definitely for mine or group of mines (*intra fines metalli Vipascensis*) and there is considerable evidence even in the present tablets that mining and smelting had by this time become differentiated and were carried on by entirely different sets of workers I may add that most Authorities appear to agree that the tablet now under discussion dates from about the end of the first century of our era

The first clause shows that there was an auctioneer appointed for the mining camp who had to conduct all sales by auction and it fixes the auctioneer's commission at one per cent which is to be doubled if not paid within three days any sales by auction ordered by the Warden must be conducted by the auctioneer gratuitously The next clause fixes in the same way the payments to be made to the public Crier one of the interesting details is that when a mine shaft is sold by order of the Warden the purchaser must pay the Crier's commission of 1 per cent

The third clause sets out the obligations incumbent upon the farmer of the baths of keeping them in good order with a proper supply of hot water specifies the bathing hours for men and women and the payments to be made for the use of the baths which are however to be free for public servants soldiers and youths under age I may remind you that it was not until 1911 that legislation in this country made any provision for the establishment of baths in connection with collieries so that in this respect the Romans were 1 800 years ahead of us—and yet we pride ourselves on our national cleanliness!

The next three clauses regulate the conditions under which cobblers, barbers and fullers may exercise their respective trades

The next clause is decidedly difficult to interpret it states that anyone engaged in cleaning, dressing or reducing copper or silver slags in the district shall declare how many men he has engaged in this work, and shall make a monthly payment for each man to the farmer (apparently the farmer of the smelting rights) and that payments shall also be made to him by anyone importing copper or silver slags into this mining camp. The clause is headed "*Scripturæ scaurariorum et testariorum*" and the latter word appears to offer some difficulty. I am inclined to think that it is quite probable that the Romans refined silver here, and that the word applied to the men working at the "test" or cupel upon which such refining would be carried out. The Latin "testa" appears to have originally meant a flat shell, a potsherd or a shallow earthenware vessel, and such a vessel containing a layer of ashes would probably be used for silver refining; in the earliest descriptions of silver cupellation that have come down to us, we find that a vessel of this description was so used; Agricola, writing in the 17th century, calls it *testa*, and the word "test" is still applied to-day to the similar vessel used in the English cupellation hearth. There is of course ample evidence that the Romans were familiar with the method of cupellation.

The eighth clause is one which I am sure will command the sympathies of many here present. I should like to draw the special attention of the Chancellor of the Exchequer to it, in order that he may see how far he has fallen in true civilisation below the legislative practice of 1,800 years ago, for this clause enacts that within the mining district teachers shall be exempt from taxation.

The last clause, unfortunately unfinished, refers to the penalties to be paid by anyone who "jumps" another man's shaft or shaft site, contrary to the mining law "*e lege metallis dicta.*"

It will be seen that there is an immense amount of interesting matter in this tablet. It is, however, far inferior in importance from the mining point of view to the second tablet, found thirty years later under similar conditions, This is evidently not a portion of the series to which the first belongs, as it differs widely from it both in matter and in style. In all probability it is a portion of the "*lex metallis dicta*" above referred to, and it appears from the inscription itself that it was given under the reign of Hadrian (117-138 A.D.). It has been conjectured with much probability that this *Lex dicta* applied to mines throughout the Colonies of the Roman Empire, possibly with minor qualifications to suit local conditions, but that its general lines were the same for all, and that it was drawn up in Rome itself and was distributed to the various colonies affected by it. If this view is correct, we have here an example of the legislation that governed mining in Great Britain 1,800 years ago.

The first line of this tablet begins in the middle of a sentence and deals with the penalties to be imposed upon anyone who smelts any ore before he has paid the price thereof as previously stated (and which we do not know, as the conditions were evidently contained in the tablet preceding the one that has come down to us). The penalty is that the owner of the shaft who commits this breach of the law shall have his share of the shaft confiscated and that the Warden shall put the whole shaft up for sale. Furthermore the informer who proves that ore has been smelted before the owner has paid the price of the half share belonging to the State, shall receive a fourth part as a reward. It is important to note that two different words, namely, "*occupator*" and "*colonus,*" are used in this paragraph indifferently as meaning the person in possession of the shaft; these words are used repeatedly throughout this inscription and apparently without any difference of meaning. I shall translate them both by the term "mine owner," although it is obvious that there is no ownership in the sense in which we use that term.

The rest of the inscription is sufficiently important to deserve translation in full, as follows:—

> Para. 2. Silver bearing shafts shall be worked in the manner contained in this law; the prices thereof

according to the generosity of the Most Sacred Emperor Hadrian Augustus shall be determined in such a manner that the ownership of that share that shall belong to the Treasury may belong to him who first offers the price for the shaft and pays into the Treasury the sum of 4,000 Sesterces.

Para. 3. Whosoever out of the number of five shafts shall have sunk one down to the ore shall work without intermission in the others as is written above; unless he shall do so others shall have the power of occupying the same.

Para. 4. If anyone after 25 days given to preparation for the expenses shall have forthwith commenced to carry out some work but shall afterwards have ceased from working for ten consecutive days, others shall have the right of occupation.

Para. 5. A shaft having been sold by the Treasury and having lain idle for six consecutive months, others shall have the right of occupation provided that when ores are drawn from the same, one-half share shall according to custom be reserved to the Treasury.

Para. 6. The owner of the shafts shall be allowed to have such partners as he may desire, provided that the latter shall contribute to the expenses for that share by which each is a partner. Should he not do so, he who has made the disbursements shall for three successive days in the Forum, and in the most frequented parts, cause the amount of the disbursements made by him to be published, and he shall intimate by Crier to his partners that each shall contribute to the expenses according to his share. Whosoever shall not contribute, or with evil intent shall have done something so that he may not contribute, or whereby he may deceive one or more of his partners, shall be deprived of his share in the shaft and that share of the partner shall belong to those partners in proportion as they shall have paid the disbursements.

Para. 7. And those mine owners who shall have made disbursements in that shaft in which there shall have been several partners shall be entitled to recover from their partners what shall be shown to have been expended in good faith.

Para. 8. The mine owners shall be allowed to sell among each other also the shares of the shaft, which they may have bought from the Treasury and paid the price thereof, for as much as they can obtain; whosoever wishes to sell his share or to buy must make a declaration before the Warden in charge of the mines; it shall not be lawful to sell or buy in any other wise. Whosoever is in debt to the Treasury shall not be allowed to give away his share.

Para. 9. The ores lying close to the shaft mouth shall be transported to the smelting works between sunrise and sunset. Whosoever is convicted of having transported ore from the shafts after sunset shall pay a fine of 100 Sesterces to the Treasury.

(I have translated the word *officina* as smelting works; it is the origin of the French "usine," and appears to mean a works as distinct from a mine, and may here have included a refinery as well as a smelting works properly speaking.)

Para. 10. Anyone who steals ore, if a slave, shall be flogged by the Warden and sold by him under the condition that he shall remain in fetters for all time, and shall not be allowed to dwell in any mines or mining district; the price of the slave shall belong to his master; if he is a freeman the Warden shall confiscate his goods, and he shall be forbidden all mining districts for ever.

Para. 11. All shafts must be carefully stayed and supported, and the owner of each shaft must replace any decayed material by such as is new and suitable.

Para. 12. It is forbidden to touch or injure the pillars or supports left for the sake of strength or to do anything with evil intent whereby these pillars or supports may be weakened and less easy to traverse.

Para. 13. Whosoever shall be convicted of damaging a shaft, causing it to cave, or destroying the upper part, or doing anything with evil intent whereby the shaft may be rendered less firm shall, if a slave, be flogged as the Warden may determine, and be sold by his master subject to the condition that he shall not be allowed to dwell in any mines; if a freeman, the Warden shall confiscate his goods to the Treasury, and he shall be forbidden mining districts for ever.

Para. 14. Whosoever works copper shafts shall keep away from the drift that carries the water away from the mine and shall leave not less than 15 feet on either side thereof.

Para. 15. It is forbidden to damage the drift. The Warden may give permission to work a trial hole from this drift for the sake of seeking for a new mine so that such trial shall not be more than 4 feet high and wide. (In this clause there is a word "*ternagus*" which appears to be new, and is not found in the dictionaries; I have translated it as a trial working, which is evidently from the context what it is intended for, but I have no clue to the way in which this meaning is derived).

Para. 16. It is forbidden to seek for or to cut ore within 15 feet of either side of the drift. Whosoever is convicted of doing otherwise in the trial holes shall, if a slave, be flogged as the Warden may determine, and be sold by his master under the condition that he shall not be allowed to dwell in any mines; if a freeman his goods shall be taken by the Treasury, and he shall be forbidden mining districts for ever.

Para. 17. Whosoever works silver shafts shall keep away from the drift that carries the water away from the mine, and shall leave not less than 60 feet on either side thereof, and he shall keep in work those shafts which he possesses or which have been assigned to him as their boundaries shall have been set, nor shall he go beyond those, nor shall he collect waste nor make trial holes beyond the limits of the shaft assigned to him in such a way as to. . . . .

It is evident that this fragment contains a summary of a number of general laws governing the ownership of mines and the conditions under which they might be worked. It is clear in the first place that the ownership of the minerals was vested in the State; apparently the State allowed any would-be miner to sink shafts for the sake of extracting the ore, at his own expense, subject to the condition that, when he raised ore, one-half thereof was to belong to the Treasury. Probably the tribute of ore was taken in kind and smelted on account of the Treasury; this provision would make it quite intelligible why it should be forbidden to remove any ore after dark when the representatives of the Treasury would be unable to see the quantity and quality of the ore thus removed. It would further seem that if any owner of a shaft abandoned it for a certain time or did not comply with all the conditions as to payment, etc., his ownership was forthwith determined, and the entire shaft fell into the hands of the Treasury; it was then apparently put up to auction or otherwise sold, and the purchaser apparently purchased subject to the same condition, namely that one-half of the ores extracted belonged to the Treasury. In other words the owner of the mine was the owner only as long as he complied with the conditions laid down and paid royalty to the State, the royalty in this case amounting to fifty per cent. of the produce. It would appear that the State claimed the absolute ownership of the mineral, but allowed it to be worked under certain conditions, most of which are unfortunately missing from the tablet that has come down to us. In the first place it may be conjectured that anyone, or possibly any settler in the mining district, wishing to mine, would be allowed to stake out a claim, probably of a certain defined area, and to mark upon it his proposed shaft sites. In most countries to-day where the State owning the mineral allows claims to be pegged out in this way, the claim-holder is bound to execute a certain amount of work in order to make good his right; it would appear that under these laws the amount of such assessment work was fixed by the Romans at sinking five shafts down to the ore. After he had done this work, the claim-holder probably became the absolute owner of the claim. If he failed to do his assessment work his right lapsed and others could take it over, or in Australian phraseology "jump his claim." Until he had completed his assessment work he must keep at it continuously, a stoppage of ten successive days rendering his claim liable to be jumped; it may be conjectured that under certain conditions that have not come down to us, the claim reverted to the State, and was then put up for sale by auction. If these views are correct, the ownership of a mining claim and of the shafts upon it could be secured either by staking out and doing the requisite amount of work, by jumping a derelict claim and completing the assessment work, or by purchase from the State. In all cases the owner held the mines subject to the condition of paying to the State one-half of the produce of the mine. The first clause, unfortunately imperfect, is by no means easy to understand; it may perhaps mean that the mine owner could purchase from the State the royalty rights by the payment down of certain sums, and in such case would be entitled to dispose of the whole of the proceeds of his mine, but this interpretation is by no means devoid of difficulties. It is possible that some such distinction may be implied in the use of the two words *occupator* and *colonus*, both of which appear to mean mine owner, though probably implying some difference in the mode of ownership. On the other hand *colonus* may mean a man who has been settled within the limits of the mining camp, and it is probable that these last alone had the right to peg out mining claims; if this is the correct interpretation, every *occupator* must be a *colonus*, though a *colonus* would not necessarily be an *occupator*. The meaning of the word *colonus* as applied to land is fairly well known; the *colonus* was a freeman, who enjoyed fixity of tenure in respect of the land he cultivated, but was bound to that land upon which he was settled; he had to pay a proportion of the produce of the land to the proprietor whoever he might be; if these conditions are transferred to a mining property, it might be deduced that the *colonus* of a mine enjoyed the absolute right of ownership of the mine, but he had to remain a miner and had to pay a proportion, here fixed at one-half, of the produce of the mine to the State as his Over-lord.

It may be noted that there is no mention here of any compensation for the owner of the surface. It is, however, quite conceivable that in such a mining district the State may have reserved all surface rights to itself, and that the mineral royalty was deemed to include payments for surface rights. It was not until much later than the date of these tablets that we find in Roman law any recognition of the rights of the owner of the surface, although ultimately the miner had to pay to the owner of the surface one-tenth of the produce, the royalty to the State having by that time been reduced to that amount (A.D. 382. See Justin. Codex Lib. XI., Tit. 6, iii.).

For us in this country perhaps the most interesting portion of the inscription is to be found in sections 6, 7 and 8, in which is laid down with the utmost clearness the basis of the system of mining partnerships, which is known in this country as the Cost Book System; this existed in full vigour for centuries in the Stannary districts of Cornwall and the adjoining districts of Devonshire, and is perhaps not yet extinct there even to-day; Cost Book Companies have indeed been started in other parts of the kingdom, but always as copies of the Cornish system. The Cost Book System is definitely a purely Cornish institution.

The general principles of the Cost Book Company are briefly as follows: a number of adventurers, as they are called, take a lease of a mineral sett, and form themselves into a Company, each adventurer taking up one or more shares, on each of which he pays an agreed sum; the number of shares is usually some multiple of 8, 64 being a very common number. An official, who may or may not be one of the adventurers, is appointed to take charge of the administration of the Company, he being known as the purser; generally a separate mine manager, called the mine captain, is also appointed. The purser keeps a book known as the cost book, into which he enters the names and respective shares of each adventurer and an account of the moneys expended on the mine and of the receipts derived from sales of ore. The adventurers hold meetings, usually once a month, at which dividends or calls, as the case may be, are decided. Any adventurer may at any time relinquish his shares provided that he has discharged all his liabilities to the Company. If he does not pay the calls made upon him his shares are forfeited to the Company. The Cost Book principle has been fully admitted in English law and has repeatedly been the subject of special legislation; practically all Cost Book cases used to be tried before the Cornish Stannary Court. It will be noted that this is a primitive type of limited company, especially suited to the development of small and irregular mines by a small group of persons all of whom were well known to each other; it will also be noted that this form and even the phraseology employed resemble closely those under which ships were owned and worked from Medieval times onward, and it may be conjectured that it received its present form in the Middle Ages. In view, however, of the fact now ascertained that the basal principles were laid down by the Romans, it can hardly be doubted that the Cornish Cost Book system has come down to us from Roman times and that the Romans were its originators.

It is noteworthy that we find evidence of this form of mine partnership wherever mining was practised under the Roman Empire. It obtained throughout Central Europe and there is abundant evidence of its existence there; the more modern German term for such a company is Gewerkschaft, each Gewerkschaft being divided into parts (frequently 64) known as "Kuxe"; the word Kux is said to be derived from a Bohemian word meaning a piece and to have been in use since the sixteenth century. The origin of the word "Gewerkschaft" is more remote; the earliest application of it that I can find is in one of the collection of mining regulations published by the Bishops of Trent dated 1208; in another of these regulations published in 1214 it is defined in the phrase "*Quattuor werki, silicet socii affidati*," which would appear to imply that it was then relatively new. In Spain again, although the successive Gothic and Arabic invasions render it difficult to follow anything like a continuous system of mining regulations, it would appear that the same principles of mining partnership must have obtained. The first complete Spanish Mining Code may be said to be contained in the Ordenanzas of the Novisima Recopilacion of Philip II. in 1584, which applied both to Spain and to the Spanish-American Colonies. Existing South American Mining Codes, *e.g.*, that of the Argentine dated 1886 and of Chile dated 1888, have followed this old code rather closely, and both have a section (Title XI.) devoted to mining companies, in which the Roman principles are exactly carried out, though naturally in greater detail. There is accordingly no reason to doubt that these regulations applied to mining in all Roman Colonies and therefore in Britain also. It is furthermore easy to understand why they should have survived in this country in Cornwall alone, seeing that the original Saxon invasion never reached this part of Britain; it may fairly be claimed that Saxon influence never made itself felt in Cornwall before the ninth century, and it is highly improbable that Roman legislation would have disappeared there before the Norman Conquest, which probably served to establish it in all its essentials. It may further be pointed out that the Roman principle of the State ownership of minerals survived in Cornwall inasmuch as there the ownership of all minerals was claimed by the Crown. Whereas in the rest of England such claims were fiercely contested until they were settled in 1568 by the Great Case of Mines, they never appear to have been questioned in the Stannary area of Cornwall and the adjoining parts of Devonshire. There are continuous records from 1198 until Edward III. granted these minerals to the

Black Prince in 1339, which show that the Crown claimed full ownership for them. An important document in this connection is the well known Tinners' Charter of King John in 1201, which was, however, a confirmation rather than a creation of such rights; it sets forth the rights of "*Stannatori nostri*" to dig for tin ore and to do various things incidental to such digging "*sicut de antiqua consuetudine consueverint*," and asserts definitely the ownership of the Crown "*in stannariis illis quae sunt dominica nostra.*" The rights of the tinners to work tin and to be subject only to the Warden of the Stannaries "*custode nostro Stannariarum nostrarum*" reminds us forcibly of the rights of the Roman Colonus and it may fairly be suggested that the system of mine ownership, which the Aljustrel tablets illustrate, continued in Cornwall unbroken from Roman times onwards.

This constitutes to my mind the chief interest of the Bronze Tablets of Aljustrel; I hold that they contain a portion of the laws under which mines were administered in Britain in Roman times, and I have submitted to you what I think is good presumptive evidence that these principles of Roman mining legislation obtained in Cornwall, and that many of them, though naturally modified in the course of ages, have been maintained in principle in Cornwall up to the present day.

THE SOLUTION OF CUBIC AND QUARTIC EQUATIONS WITH NUMERICAL COEFFICIENTS

BY A S PERCIVAL

[Read February 24th 1921]

THE SOLUTION OF CUBIC EQUATIONS BY CIRCULAR FUNCTIONS

Given $Ax^3+Bx^2+Cx+D=0$, put $x=\frac{y-B}{3A}$ to eliminate the second term and to replace the leading coefficient by unity The transformed equation is $y^3+py+q=0$,

where $p=9AC-3B^2$

and $q=2B^3-9ABC+27A^2D$

It will then be found that if the original coefficients A, B, C, and D be integral, p and q will be also integral

(If A= 1 and $\frac{1}{3}$B be integral say n, it is simpler to put $x=y-n$, and the transformed equation is $y^3+py+q=0$, where $p=C-nB$ and $q=2n^3-nC+D$)

Regarding p and q as signless find $\dfrac{\frac{q}{2}}{\frac{p}{3}\sqrt{\frac{p}{3}}}$, and call it k, so that k is signless in every case

In the solution of $y^3+py+q=0$, it will be observed that a different method must be used according to the sign of p, and hence it is advisable to consider p as signless, but to regard q as carrying its appropriate sign, so we write $y^3\pm py+q=0$

I —For the case $y^3+py+q=0$, put $k=\cot\psi$, and

$$y_1=-\sqrt{\frac{p}{3}}\left\{\left(\cot\frac{\psi}{2}\right)^{\frac{1}{3}}-\left(\tan\frac{\psi}{2}\right)^{\frac{1}{3}}\right\}$$

This root y_1 is the only real root and it carries the opposite sign to that of q, which is indicated by the sign — The remaining roots y_2 and y_3 are given by $-\frac{1}{2}y_1 \pm \left(\frac{1}{4}y_1^2+q/y_1\right)^{\frac{1}{2}}$,

$$\text{or} \qquad -\frac{y_1}{2} \pm \sqrt{-p-3\left(\frac{y_1}{2}\right)^2}.$$

The term q/y_1 is always negative, for in both cases q must carry the reverse sign to that of y_1.

II.—For the case of $y^3-py+q=0$, there are three subdivisions depending upon the value of k.

(i) When $k>1$, put $k=\operatorname{cosec}\psi$; then

$$y_1=-\sqrt{\frac{p}{3}}\left\{\left(\cot\frac{\psi}{2}\right)^{\frac{1}{3}}+\left(\tan\frac{\psi}{2}\right)^{\frac{1}{3}}\right\},$$

and the two unreal roots y_2 and y_3 are given by

$$-\frac{y_1}{2} \pm \sqrt{p-3\left(\frac{y_1}{2}\right)^2}.$$

(ii) When $k=1$, $y_1=-2\sqrt{\frac{p}{3}}$ and $y_2=y_3=\sqrt{\frac{p}{3}}$

Here y_1 as always takes the opposite sign to that of q, but now the other roots take the same sign as q.

(iii) When $k<1$, put $k=\cos 3\phi$,

$$\text{then} \qquad y_1=-2\sqrt{\frac{p}{3}}\cos\phi$$

$$y_2 \text{ and } y_3=-2\sqrt{\frac{p}{3}}\cos(\phi\pm120^\circ).$$

(If tables are not at hand, the following method which is given in Barlow's tables is a very good approximate method. Given $y^3+py-q=0$, let a be an approximate root. Write $v=a^3+pa$, then $y_1=a+\frac{a(q-v)}{2a^3+q \text{ or } v}$ approximately, where q is used, unless $a<1$ when v is used. As a numerical example consider $y^3+3y=6$. Taking $a=1{\cdot}3$, a first application of the method gives $y_1=1{\cdot}288$. Taking $a=1{\cdot}288$, a second application gives $y_1=1{\cdot}2879097$. These eight figures are correct. On using the method of circular functions with 7 figure logarithms one obtains 1·28791, a nearer result with one operation, which illustrates the capacity of each procedure. The value of the root correct to 9 figures is 1·28790976. Now in this case

$y^3+3y=6$, one could easily find the first three figures from Molesworth's pocket-book which gives tables of squares and cubes for numbers of three figures, but it would be troublesome if p entailed many figures. In such a case put $y=z\sqrt{\frac{p}{3}}$; then

$$y^3+py-q=\frac{p}{3}\sqrt{\frac{p}{3}}z^3+p\sqrt{\frac{p}{3}}z-qz=z^3+3z-\frac{q}{\frac{p}{3}\sqrt{\frac{p}{3}}}=0$$

and an approximate value of z can be easily estimated from the tables.)

The explanation of the method by circular functions is now quite simple. We have three fundamental formulæ—

I. . . . . . . $4\sinh^3 u+3\sinh u=\sinh 3u$.
II. (i.) . . . . $4\cosh^3 u-3\cosh u=\cosh 3u$.
II. (iii.) . . . $4\cos^3\phi-3\cos\phi=\cos 3\phi$

In class I., if instead of putting $y=z\sqrt{\frac{p}{3}}$, we replace z by $2\sinh u$, the last equation in z becomes

$$8\sinh^3 u+6\sinh u-\frac{q}{\frac{p}{3}\sqrt{\frac{p}{3}}}=0,$$

and so $\sinh 3u=\frac{\frac{1}{2}q}{\frac{p}{3}\sqrt{\frac{p}{3}}}$ or k.

But in I. we put $k=\cot\psi=\frac{\cot^2\frac{\psi}{2}-1}{2\cot\frac{\psi}{2}}=\frac{1}{2}(\cot\frac{\psi}{2}-\tan\frac{\psi}{2})$

$$=\sinh 3u, \text{ if } \cot\frac{\psi}{2}=e^{3u}$$

and $y_1=2\sqrt{\frac{p}{3}}\sinh u=\sqrt{\frac{p}{3}}\left\{\left(\cot\frac{\psi}{2}\right)^{\frac{1}{3}}-\left(\tan\frac{\psi}{2}\right)^{\frac{1}{3}}\right\}$.

Similarly in class II. (i.), $k=\operatorname{cosec}\psi=\frac{\cos^2\frac{\psi}{2}+\sin^2\frac{\psi}{2}}{2\sin\frac{\psi}{2}\cos\frac{\psi}{2}}$

$$=\frac{1}{2}\left(\cot\frac{\psi}{2}+\tan\frac{\psi}{2}\right)=\cosh 3u,$$

so $z=2\cosh u$ and $y_1=2\sqrt{\frac{p}{3}}\cosh u=\sqrt{\frac{p}{3}}\left\{\left(\cot\frac{\psi}{2}\right)^{\frac{1}{3}}+\left(\tan\frac{\psi}{2}\right)^{\frac{1}{3}}\right\}$

In class II. (iii), where $k=\cos 3\phi$, $z=2\cos\phi$ or $y_1=2\sqrt{\frac{1}{3}p}.\cos\phi$. It will be noted that ψ is the complement of the gudermannian θ, so that

$$\sinh v=\cot\psi=\tan\theta \qquad e^v=\tan\left(\frac{\pi}{2}-\frac{\psi}{2}\right)$$
$$\cosh v=\operatorname{cosec}\psi=\sec\theta \qquad =\tan\left(\frac{\pi}{4}+\frac{\theta}{2}\right)$$
$$\tanh v=\cos\psi=\sin\theta$$

THE SOLUTION OF QUARTICS WITH NUMERICAL COEFFICIENTS.

Given $Ax^4+Bx^3+Cx^2+Dx+E=0$,

Put $x=\frac{y-B}{4A}$, so as to eliminate the second term and replace A by 1, when $y^4+Qy^2+Ry+S=0=f(y)$ is obtained;

$$\text{where}\quad Q=16AC-6B^2$$
$$R=8(B^3-4ABC+8A^2D)$$
$$S=16A(B^2C-4ABD+16A^2E)-3B^4.$$

Clearly we may assume $y=\frac{1}{2}(\alpha\pm\beta)$ or $\frac{1}{2}(-\alpha\pm\gamma)$

$$\text{or}\quad f(y)=\{y^2-\alpha y+\tfrac{1}{4}(\alpha^2-\beta^2)\}\{y^2+\alpha y+\tfrac{1}{4}(\alpha^2-\gamma^2)\}.$$

The auxiliary cubic is $\phi(z^2)=z^6+2Qz^4+(Q^2-4S)z^2-R^2=0$

$$\text{or}\quad (z^2-\alpha^2)(z^4+pz^2+q)=0.$$

As the last term $(-R^2)$ is negative there must be at least one positive root, and it will be a square number if the solution is integral; denote it by α^2. If more than one positive root, it is convenient to denote the highest number by α^2.

$$\beta^2=-\frac{2R}{\alpha}-2Q-\alpha^2 \qquad \gamma^2=\frac{2R}{\alpha}-2Q-\alpha^2.$$

Example (i.) $\quad 2x^4-11x^3+x^2+50x-24=0.$

Put $x=\frac{y+11}{8}$, then $y^4-694y^2+2856y+51597=0.$

The auxiliary cubic is $z^6-1388z^4+275248z^2-8156736=0.$

Here $\alpha^2=1156$; $\alpha=34$ and hence $\beta=8$, $\gamma=20$. Then the roots for y are 21, 13, -7, -27 and finally $x=4$, 3, $\frac{1}{2}$, or -2.

Another method, which I owe to the late Mr. J. H. Gurney, is simpler in most cases. Write the auxiliary cubic in factors $(z^2-\alpha^2)(z^4+pz^2+q)=0$. Then

$$\{y^2\pm\alpha y+\tfrac{1}{4}(\alpha^2+p-2\sqrt{q})\}\ \{y^2\mp\alpha y+\tfrac{1}{4}(\alpha^2+p+2\sqrt{q})\}=0,$$

and solve the two quadratics In this case q must be regarded as signless and $\sqrt{q}$ is positive and real whereas p carries its sign

The upper signs must be taken when R is positive the lower signs when R is negative

Example (i) as before $y^4-694y^2+2856y+51597=0$ This leads to $(z^2-1156)(z^4-232z^2+7056)=0$ and hence

$$f(y)=(y^2+34y+189)(y^2-34y+273)$$

Solving the quadratics we obtain the roots as before

Example (ii)

$$x^4+4x-1=0$$

$$\phi(z^2)=z^6+4z^2-16=0=(z^2-2)(z^4+2z^2+8)$$

$$f(x)=(x^2+x\sqrt{2}+1-\sqrt{2})(x^2-x\sqrt{2}+1+\sqrt{2})$$

Therefore $x=-\frac{1}{2}\sqrt{2}\pm\sqrt{\sqrt{2}-\frac{1}{2}}$ or $\frac{1}{2}\sqrt{2}\pm\sqrt{-\sqrt{2}-\frac{1}{2}}$

The two real roots are 249038376 and -1 663251938 and the two unreal roots are 707106781 $\pm\sqrt{(-1\ 914213562\ )}$

Note —(1) Whenever the cubic has 3 real positive roots the quartic has 4 real roots

(2) If the cubic has 1 real positive root and two unreal roots the quartic has 2 real and 2 unreal roots

(3) If the cubic has 1 real positive root and 2 real negative roots the quartic has either 2 real and 2 unreal roots or it has 4 unreal roots

"LIMITS OF ACCURACY IN ENGINEERING MANUFACTURE."

By S. J. DAVIES, B.Sc., A.M.I.Mech.E.

[Read February 21st, 1921.]

There are errors in *all* acts of measurement. These depend partly on the means used and partly on the degree of human skill exercised. Error must, therefore, be "tolerated" to a greater or less degree. A difference of five yards in the lengths of two football pitches is hardly noticeable; an error of inches in the length of a cricket pitch, although an extremely small percentage of the twenty-two yards specified, is, however, of great moment. In the world of engineering the error tolerated may vary from the 'near enough" measurement at one end to the "ten-thou," *i.e.*, one ten-thousandth of an inch, discussed daily in the gauge or tool department of most works engaged on engineering manufacture in quantities.

In this type of engineering, commonly termed "mass production," the quantities are large with relation to the value of a unit, as distinct from the large unit met with in the constructional or engine-building branches. Electrical switchgear, aero and automobile engines, typewriters, shells and guns may be mentioned as examples of engineering manufacture in quantities. And for economical production some degree of interchangeability between all examples of the same part, when machined, is essential. The machining of parts is sub-divided into a number of distinct operations, and a certain accuracy must often be attained in early operations, since the part is often located in jigs or fixtures for later operations in such a way as to make use of earlier machined dimensions. Parts machined to a high degree of accuracy can be assembled easily and cheaply, since hand-fitting is largely eliminated. The supply of spare parts is also facilitated.

In order to achieve the degree of interchangeability required, it becomes necessary to establish, for each dimension of a part, certain " limits " of accuracy between which this must lie; in other words, the error which may be tolerated for each dimension is fixed. In this connection it must be remembered that accuracy costs money, and after a certain essential tolerance has been met, further efforts are unnecessary and wasteful. To measure accurately a particular dimension is a lengthy, and therefore costly, operation; the use of limit gauges render practical measurement economical, since, if the dimension falls between the limits fixed by the gauges, nothing more is demanded.

The consideration of manufacturing limits falls under five headings or divisions. These are (*a*) measurement of diameters of cylindrical parts; (*b*) measurement of lengths, including cumulative dimensions; (*c*) measurement of distances apart of the centres of holes; (*d*) measurement of screw threads; (*e*) measurement of special shapes such as tapers, splines, etc. Of these (*a*), (*b*), (*c*) and (*e*), will be treated in the Paper.

(*a*) Diameters of cylindrical parts.—Consideration of these may be regarded as fundamental in the general study of limits of manufacture.

In every pair of parts mating together one must be selected as a basic one whose dimension is to be regarded as accurate, within the degrees of accuracy demanded by the work, and of the nominal size; the other part, also machined within the required degrees of accuracy, is given a definite variation from the nominal size in order to attain a certain kind of mating, or " fit," as it is termed in the workshop. It may be as well to point out that the " tolerance " is the total error in manufacture which can be permitted, while the definite difference between the dimensions of two-parts mating together is called an " allowance." It will be understood later that certain tolerances are given according to the class of work while the allowances are arranged to produce certain kinds of fit.

Several years ago, based largely on the demands of mill engineering, the British Engineering Standards Association recommended that shafts or plugs should be made, within appropriate limits, to the nominal diameters, while the corresponding holes should have the required allowance or variation from standard. This is termed the " shaft basis." In later times, however, the opposite of this has come into practice, viz., the " hole basis " in which the hole is to the nominal dimension and the shaft is given the necessary allowance. The reason for this change lies in the fact that holes are generally made by means of drills, reamers, or cutters, whereas plugs and shafts are turned or ground: the former tools are standardised and produce a diameter which is relatively fixed; parts produced by turning or grinding can more easily be given an allowance from the nominal dimension. Further, holes can be most conveniently machined to limit plug gauges, and in this case direct measurement of the hole, always very difficult, is not necessary, while, for shafts measurement by means of a micrometer is readily carried out. Thus, in cases where rigid economy must be considered, a set of standard plug limit gauges only is essential—if the shaft basis were used, plugs covering all recognized allowances would be necessary to ensure the holes being to the correct size.

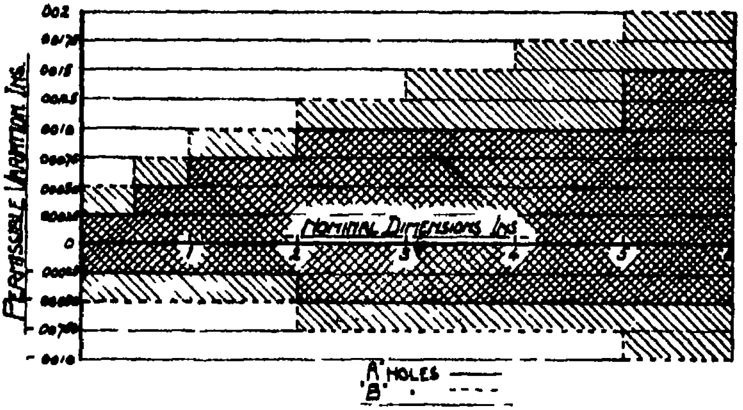

FIG. 1.

There are three systems of tolerances and allowances in use, and, in addition, most manufactures provide examples where special conditions have to be met. The three systems do not differ greatly in the tolerances and allowances arranged but only in the manner in which these are arranged. The Newall System, in fairly common use in this country, although not put forward as a perfect one, will be discussed, in order to keep the Paper within the desired bounds.

Fig. 1 shows the tolerances allowed for holes under this system—it will be remembered that holes are basic, and should therefore, be as correct as is commercially possible The tolerance increases in steps to suit practical demands, but follows the general law: Tolerence $= a + b\surd$ (Diameter),

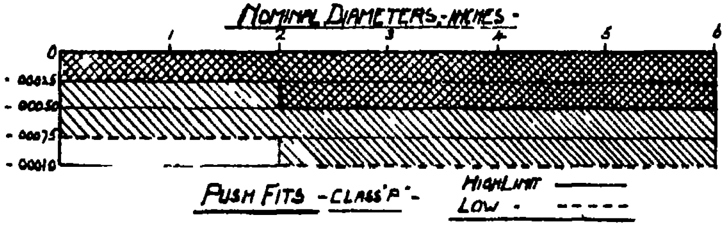

FIG 2

where a and b are constants. Two classes of tolerances "A" and "B" are given in the figure, and it will be noticed that the tolerances are arranged so that two-thirds are above the nominal dimension and one-third below the nominal dimension. In making a hole the workman hugs the lower limit rather than the upper, while in making a plug or shaft the upper limit is more often approached, since in the event of error in these cases, the part may be rectified.

Fits may be divided under three divisions, clearance, interference, and transition. Clearance fits are necessary when relative motion is to take place between the parts, and include "Running fits" and "Push fits." Figs. 2 and 3 show, in a diagrammatical form, the allowances arranged for one class of push fit, "P," and three classes of running fits, "*X*," "*Y*," *and* "*Z*." It will be noticed from a comparison of these figures with Fig 1 that there is always a positive allowance or clearance between the hole and its shaft Thus for a hole and shaft of 2½ inches diameter nominal the smallest diameter of the hole in class A is 2 4995 inches while the greatest diameter of the shaft is 2 499 inches for the highest class 7 of running fits The essential feature of a clearance fit is that motion may take

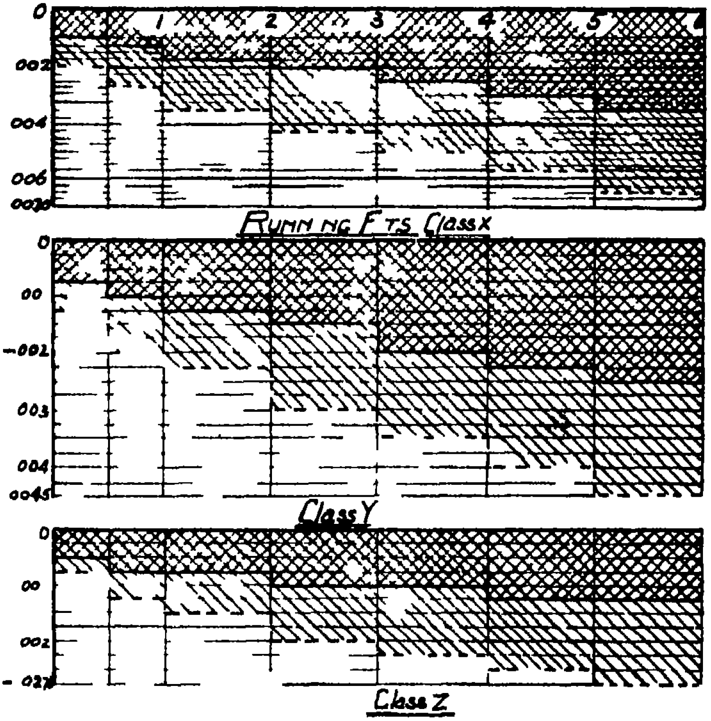

Fig 8

place freely and with sufficient allowance for proper lubrication Wear will take place and must often be considered in connection with the permissible allowance

Interference fits comprise Drive fits Force fits and Shrink fits These include all cases where the two parts are to be rigidly connected so as to ensure complete lack of movement under service conditions With this class of fit there must always be some 'interference' between the two parts, i.e. the shaft must always be larger than the corresponding hole—the allowance therefore will be negative. Suitable allowances for drive and force fit are arranged under the Newall system; these are shown in Fig. 4. Shrink fits are not standardised since the demands of engineering productions are so varied in this respect. A curve of thermal expansion for steel is given

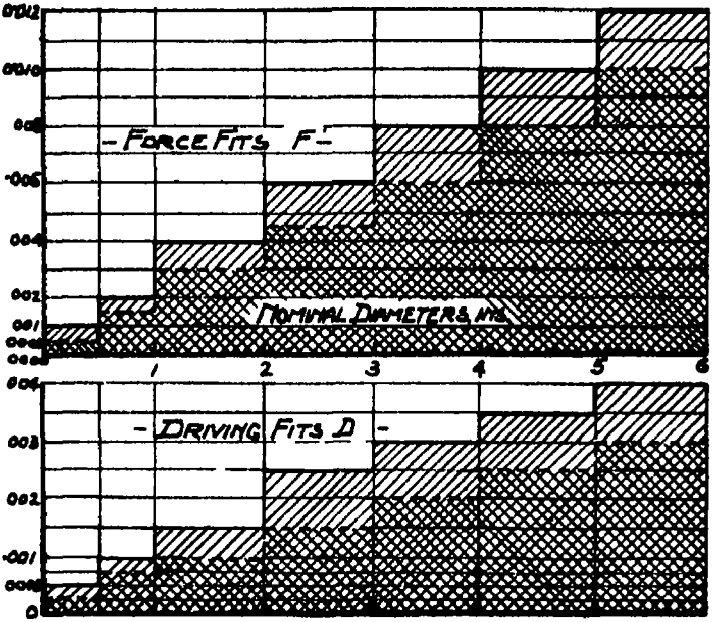

FIG. 4

and allowances for shrinkage can be arranged from such a curve; allowances should be of an order so that when cold the interference between the parts exceeds that of a force fit. To the reader who is not familiar with workshop expressions it may be added that a 'light drive fit' is one where the parts may be mated by light blows from a hand hammer; a drive fit is closer than this; a force fit demands a force of the order of that supplied by a screw or hydraulic jack. Shrink fits, of course, make use of the expansion and contraction of one of the parts by heating and cooling in order to obtain the desired grip. Parts assembled to a force fit should not necessarily be destroyed or damaged in taking apart again.

The third class of fit, transition fits, includes those which lie between clearance and interference fits. The essential condition is that the parts pair together with a complete absence of relative motion but with a proper registration of one with another. They must further permit of assembly and taking apart without difficulty or damage to them. No wear, of course, may be expected from this class of fit. The British Engineering Standards Association classifies light drive and key fits as transition fits. The Newall system does not provide definitely for them, although the allowances of the class P, push fits, apply to some extent as transition fits. With the stepped increases of tolerance and allowance rendered necessary by practical conditions some confusion is inevitable on the border lines between the classes. If it were possible to arrange systems without steps, all demands would be met by equations of the order $Y = a + b\sqrt{}D.$, giving suitable values to the constants a and b.

(*b*) Lengths, including cumulative dimensions. When simple lengths up to about 4 inches are considered, their tolerances and allowances follow closely those given for the diameters of cylindrical parts as regards clearance, interference, and transition fits. For longer lengths, however, tolerances have to be increased somewhat owing to the practical difficulties of manufacture and measurement. The cylinder is the simplest form to machine and to measure; lengths are more difficult with the ordinary workshop equipment.

There are many examples of manufacture, placed under this heading which, further, do not lend themselves to standardisation. These are often of the type, which may be called cumulative, in which individual dimensions cannot be considered alone but only with reference to other dimensions related to them In order to make this more intelligible the author would offer some examples from practice Fig 5 shows the big end of the connecting rod of an aero engine in position on its corresponding crank-pin The fit on the diameters of the journal and bearings

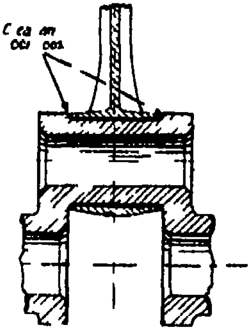

FIG 5

is obviously a running fit governed by the conditions of (*a*) there must also be clearance between the sides of the bearing and the ends of the crank pin journal In practice the working was found to be satisfactory if 002 to 004

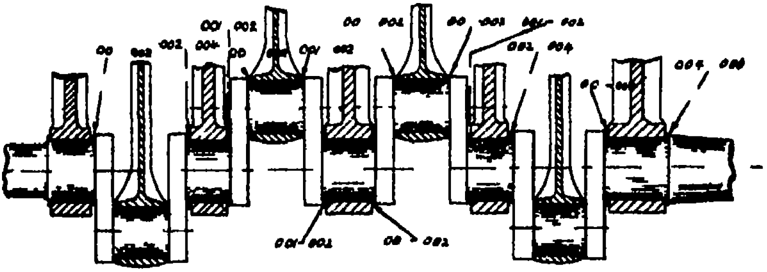

FIG 6

inches were allowed as total side-clearance or float '
This is a case which permits of ready measurement

Fig 6 shows a more difficult case viz the allowances which must be given on the relative lengths of the crank-shaft and engine body or crank case in order to compensate for their difference of expansions on heating up in running—in this example the crank-case expands more than the shaft. The tolerances on these dimensions are of the order of plus or minus ·001 inches, while the allowances increase by ·002 inches for each bearing passing from the centre bearing towards the ends—in this case location endwise of the shaft in the case is at the centre bearing; in some engines one of the end bearings is the locating one and allowances must be given starting from this one. It will be appreciated that in this example the allowances are cumulative, but the tolerances cannot be, as, if they were, the allowances would be nullified. So that the tolerances on the overall lengths, although logically the sum of the individual tolerances, have to be made much less than the sum in order to satisfy the demands of the work. The cases in which lengths and distances are cumulative form an interesting problem for the designer and manufacturer.

(c) Distances apart of the centre lines of holes. This is a most important sub-division from the point of view of practice, in which a number of cases have to be considered.

When two holes carry spindles or shafts upon which gear-wheels are mounted, the correct and satisfactory mating of the gears depends on the accuracy of the distance of the centres of the holes. The diameters of the holes will be measured to the standards given in (*a*), and by the insertion of suitable plugs, which project from the holes, the distance of the centres may easily be measured. This case is simple, but a more difficult case is introduced when a number of holes in a part have to be positioned so that their relative positions must bear a certain mutual relation, e.g., a series of holes in the flange of a cover to pass over studs in a casting. In ordinary practice it is sufficient if one set of holes be marked off by hand and drilled as nearly as possible in their correct positions; the other part is generally marked off from these holes and drilled, adjustment being made as necessary in fitting together. For economical manufacture in quantities, both parts must be drilled in jigs, and the tolerances allowable in such jigs form the real problem.

Fig. 7 shows the conditions which must be observed in order to obtain interchangeability on these holes. The holes in the cover must, in general, be what are called "clearing" holes, i.e., there is a considerable allowance, of the order of $\frac{1}{64}$ to ^{1} inches, on the diameter of the cover hole over the diameter of the corresponding stud in the casting. So, assuming the diameters of the studs and holes to be correct (their tolerances would be, say, plus or minus ·001 inches), the effect of the clearing allowance may be considered by reference to the figure. In such a case it is clearly not sufficient for the distances between each two successive holes to be considered; it is essential, also, to keep each hole, within definite limits, to its nominal position with regard to *all* other holes. Thus the problem resolves itself into deciding the variation permissible, in all directions, in the position of each hole. If D be the diameter of the holes, and d be the diameter of the studs, then $(D-d)$ is the amount of clearance allowed. If t be the maximum distance, in any direction, by which any hole may be wrongly placed, from a consideration of Fig. 7 it will be seen that $t=\frac{1}{4}(D-d)$. And, if x be the nominal distance of the centres of two holes, then the dimension must fall between the limits $(x+2t)$ and $(x-2t)$ for interchangeable production. And a tolerance of plus or minus $2t$ is the maximum which can be permitted between the centres of *any* two holes. Thus, with a casting to which is secured a cover by $\frac{3}{8}$-inch studs, the clearing holes in the cover being $\frac{3}{8}+\frac{1}{64}$ inches, the value of t becomes ·004 inches,

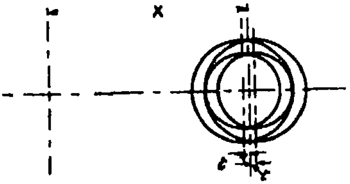

Fig. 7.

In order to produce work to this degree of accuracy, the jigs from which the holes are drilled must be sufficiently correct so that the centres of their holes are accurate within a permissible value of, say, ·0005 for t. The high standard of work in the tool room of a firm engaged on engineering manufacture will be realised when such tolerances are considered.

(*e*) **Special shapes: tapers, splines, serrations.**—Tapers form a very efficient means of mating one part with another where the two parts must be accurately concentric. When fitted with keys, they also form a satisfactory means of transmitting torque from one part to another, such as from a shaft to a gear-wheel or propeller. In order to be efficient, however, these must be very carefully made. The fitting of the hollow part on to the shaft ensures a rigid alignment of both, while the keys are only called upon to withstand pure shear; further, if the tapers are well fitted and pulled up tightly together, some of the torque may be taken by the friction of the two parts, thus lessening the demands on the key, a desirable point, particularly where the torque to be transmitted is not uniform. It may be added that, in addition to the good fit of the tapers, the key

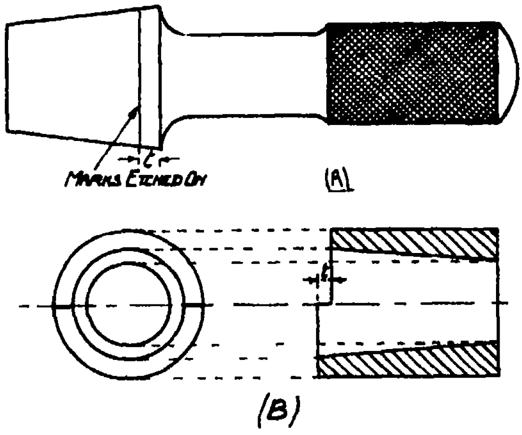

Fig. 8.

should be a good push fit or light drive fit in the sides of the corresponding keyway, but should be clear on the top.

It is necessary, therefore, in manufacturing tapered parts in quantities to lay down very definite degrees of accuracy in the taper. Magneto drives form an important example of such production, and tapers have been standardised for these by the British Engineering Standards Association. Tapers are measured as 1 inch in change of diameter in x inches length, or briefly, 1 in x. In practice, tapered work is made direct to gauges—direct measurements are not taken—but it is always desirable in checking, say, a plug under manufacture, with a taper ring gauge, to use some kind of marking, e.g., prussian blue, in order to ensure contact between them over their whole lengths, as a slight difference in the angles of taper of these is not very noticeable when checking without marking. The correctness of the angle of a taper, either plug or ring may be regarded as sufficient for practical demands if checked in this way. For checking the diameters, so that, at a particular point they fall within certain permissible limits, the gauges are marked as shown in Fig. 8, (*a*) and (*b*). The plug shown in (*a*) is marked at a distance "*t*" as shown, and, if the end of the part to be gauged falls between the etched mark and the end, the diameters of the part may be adjudged correct. Similarly, in (*b*), a projection is shown to the face of the ring gauge at distance "*t*" so that the end of the plug part to be measured should fall between this projection and the face of the gauge. From what has been said above, the value of *t* will be

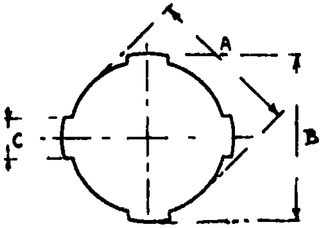

FIG. 9.

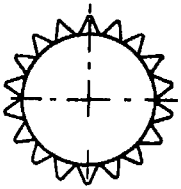

FIG. 10.

t-thousandths of an inch for a taper of 1 in *t*, if the tolerance on the diameter be ·001 inches.

Splined drives form a very important class, since rotary motion between two parts is prevented while axial motion may take place — gear boxes for automobiles and machine tools make use of splines—. Fig. 9 shows the shape of splined drives, whence it will be seen that the dimensions A, B, and C are of importance. In such a shape, it will be realised after consideration that all of A, B, and C influence the setting of the outer part relatively to the shaft as regards concentricity. In most machine parts, it is general, in practice, to allow only one dimension to give location in any one direction, but with splined shafts at least two, B and C, or A and C, are made within close limits, while the third is arranged to give ample clearance The following particulars, taken from a practical case will explain this:—

Dimension	Bore (Inches)	Newall Class	Shaft (Inches)	Newall Class	Remarks.
A	$1\cdot0000^{+\cdot00075}_{-\cdot00025}$	A	$1\cdot0000^{-\cdot00125}_{-\cdot00250}$	Y	Sliding Drive.
B	$1\cdot2000^{+\cdot000}_{-\cdot005}$	Clearance	$1\cdot2000^{-\cdot010}_{-\cdot015}$	Clearance	
C	$\cdot2800^{+\cdot00025}_{-\cdot00025}$	A	$\cdot280^{-\cdot00075}_{-\cdot00125}$	Y	
A	$1\cdot0000^{+\cdot00075}_{-\cdot00025}$	A	$1\cdot0000^{-\cdot00025}_{-\cdot00075}$	P	Fixed Drive.
B	$1\cdot2000^{+\cdot000}_{-\cdot005}$	Clearance	$1\cdot2000^{-\cdot010}_{-\cdot015}$	Clearance	
C	$\cdot2800^{+\cdot00025}_{-\cdot00025}$	A	$\cdot2800^{-\cdot00025}_{-\cdot00075}$	P	

The same kind of fit is arranged, in this case, both on dimensions A and C. The grooves in splined shafts are generally form-milled, which demands great care to achieve this degree of accuracy; the hollowed parts are, however, broached, so that, if the broach is correctly made, accurate and interchangeable production should follow.

A serrated shaft is shown in section in Fig. 10. This may be used in cases similar to those in which splines are used, except that, while it is ideally suitable for the transmission of torque, it is not eminently suitable for relative motion of the parts parallel to their axes. The methods of producing the shafts and bores are similar to those of splines, but, on account of the fact that the important surfaces are not parallel one to another, the difficulties of production of shafts are increased. The angular positions of the grooves relative to each other are even more important than in the case of splines, and the angles of the grooves also demand great attention. The measurements of serrated shafts are carried out in similar ways to those of screw threads, and, as the latter subject is to be dealt with in a coming paper, the reader is referred to that paper.

Serrations have been used for mounting gear wheels on crank-shafts of aero engines when these wheels transmit the whole power of the engine. Another example is in the British Standard Air-screw Boss which has been adopted by the Royal Air Force.

The tolerances given in the diagrams are intended to apply to the work. Gauges and other appliances are used for producing and measuring the work. These, too, must necessarily have errors and therefore tolerances must be arranged to apply to them. Such jigs, fixtures, tools, and gauges are produced, in general, in the jig and tool department of the manufacturing firm. These tolerances are, therefore, termed tool-room tolerances, and are of the order of 25 per cent of those allowed on the Newall " A " holes. The necessity for the extremely fine tolerances worked to—and sometimes thought impracticable, or at any rate unnecessary, by those not familiar with the condition of engineering manufacture in quantities—becomes obvious when the objects of the work in the tool-room are considered. In the tool-rooms, too, definite standards of comparison of length must be present. Those of Johansson are in common use in this country. Such standards are produced and tested under laboratory conditions and must, naturally, be to a much higher degree of accuracy—Johansson's are guaranteed correct to within plus or minus 00001 inches on any dimension.

The present paper, of necessity, touches only the fringe of a subject which is of the greatest moment to certain branches of engineering

SOME FEATURES IN CONDENSING PLANT OPERATION.

By G F HARDY, M Sc

[Read February 21st, 1921]

Section I.—Dependence of Condenser Efficiency on Air Pump Capacity and "Inactive Zones."

In the design of surface condensers it is usual to proportion the cooling surface either upon the Indicated Horsepower of the prime mover, or upon the probable weight of steam to be condensed per hour; and the volumetric displacement of the air pump is based upon the quantity of condensate to be extracted.

Proportions in practice are as follows:—

Table I.

Marine engines (Mercantile). Condenser surface—1·25 to 1·5 square feet per I.H.P.
Air pump capacity—1 1 cubic feet per lb. of steam condensed.

Naval practice (T.B. Destroyers). Condenser surface—·4 square feet per S.H.P.
Air pump capacity—·23 to ·25 cubic feet per lb. of steam condensed.

Turbo Electrical Power Plant. Condenser surface—2 square feet per KW. at 28″ vacuum.
2·5 square feet per KW. at 29″ vacuum.
Air pump capacity. .65 cubic feet per lb. of steam at 26″ vacuum.
·9 ,, ,, ,, 27″ ,,
1·1 ,, ,, ,, 28″ ,,

Condensing plants designed on the above proportions are essentially a compromise between efficiency and practicable dimensions or initial cost, they show on analysis evidence of extreme inefficiency.

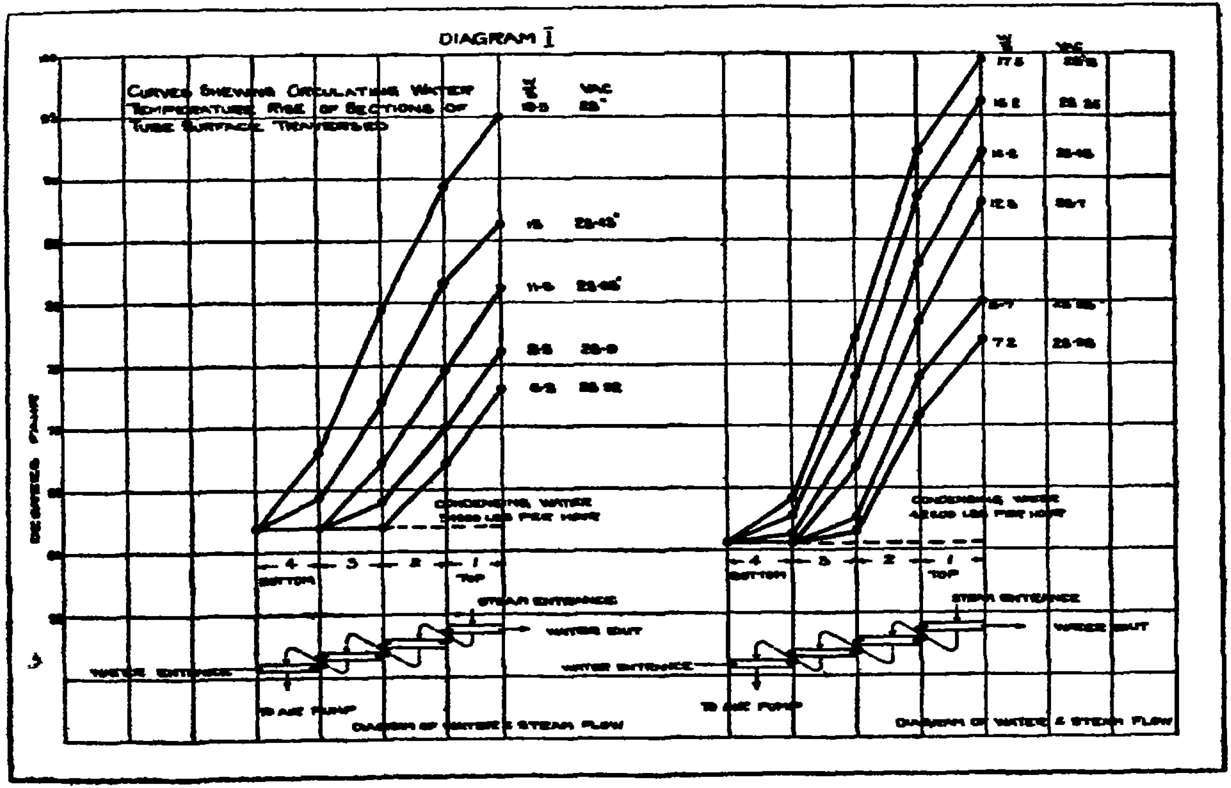
DIAGRAM I
CURVES SHEWING CIRCULATING WATER TEMPERATURE RISE OF SECTIONS OF TUBE SURFACE TRAVERSED
VAC
19·5 29"
15 28·43"
11·6 28·46"
8·6 28·9
6·5 28·92
17·5
16·2 28·25
14·6 28·45
12·5 28·7
8·7 28·55
7·2 28·95
DEGREES FAHR
CONDENSING WATER
LBS PER HOUR
4
3
2
1
BOTTOM
TOP
STEAM ENTRANCE
WATER EXIT
WATER ENTRANCE
TO AIR PUMP
DIAGRAM OF WATER & STEAM FLOW

The proportions of the air pump are principally responsible for this lack of efficiency, as this method of proportioning takes little or no account of the quantity of air which has to be extracted from the condenser. The quantity of air carried into the system is proportional to the water evaporated in the boiler, but incidental and insiduous air leakage are factors which can only be allowed for by extremely extravagant proportions in cooling surface, and in capacity of air pump.

STANDARDS OF EFFICIENCY.

The following standards of efficiencies are here adopted:—

(1) *Vacuum Efficiency*—

$$\frac{\text{Actual vacuum in condenser.}}{\text{Vacuum corresponding to the air pump discharge temperature.}}$$

(2) *Thermal Efficiency*—

$$\frac{\text{Temperature of air pump suction.}}{\text{Temperature corresponding to the vacuum.}}$$

THE TEMPERATURE RISE IN THE CIRCULATING WATER DURING ITS PASSAGE THROUGH THE CONDENSER.

The curves published by Prof. R. L. Weighton in his paper "The Efficiency of Surface Condensers"[*] show that the temperature rise of the circulating water takes place wholly or in major part in the two upper water passes, indicating that the heat transmission is confined principally to these two passes.

Prof. Josse displays this same fact, the curves on Diagram 2 being reproduced from his results.[*]

In curve 2 the exhaust steam did not contain much air whilst in curve 3/9 6 kilograms of air were allowed to leak in per hour, and about 40 per cent. of the cooling surface is absolutely inactive.

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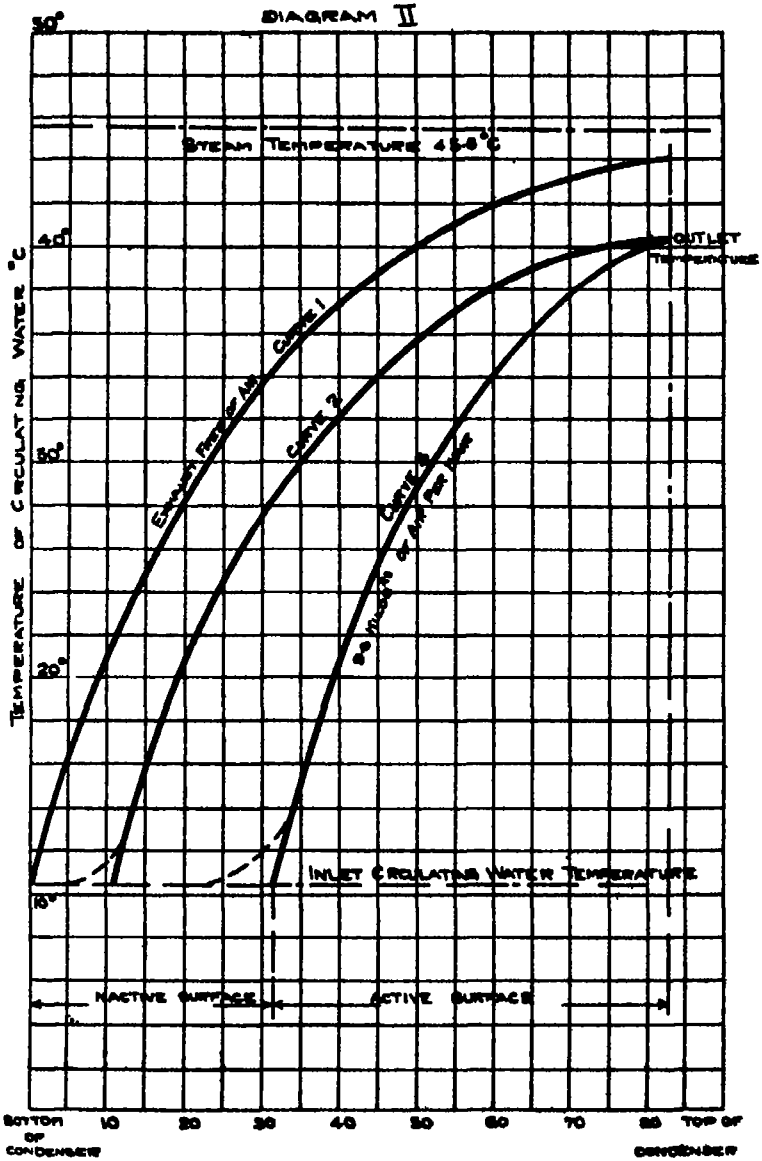
[*] *Vide Trans. Inst. Naval Architects*, 1906.

DIAGRAM II



Cooling Surface in Sq Metres.

Curves shewing Temperature Rise of Circul^{g} Water

"ACTIVE" AND "INACTIVE" ZONES.

The evidence reviewed above shows that the cooling surface may be divided into two comparatively distinct portions or zones, an upper portion which may be termed an "active" zone and a lower portion or "inactive" zone, the active zone is that one concerned in the actual condensation of the steam. The surface in this zone is highly efficient, giving an extremely high heat transmission co-efficient.

At the end of the active zone practically the whole of the steam is condensed, and with the usual percentage of air present the temperature of the air steam mixture at this point is only slightly lower than the temperature of the entering steam, but the volume per pound of the mixture is too great to be within the removal capacity of a mechanical air pump of practicable dimensions.

Herein lies the function of the inactive zone, viz., to cool the air and de-vaporise it, thereby greatly reducing its specific volume.

This concentration can only be performed by a reduction in temperature causing condensation of the remaining steam, and resulting in the mixture becoming richer in air contents.

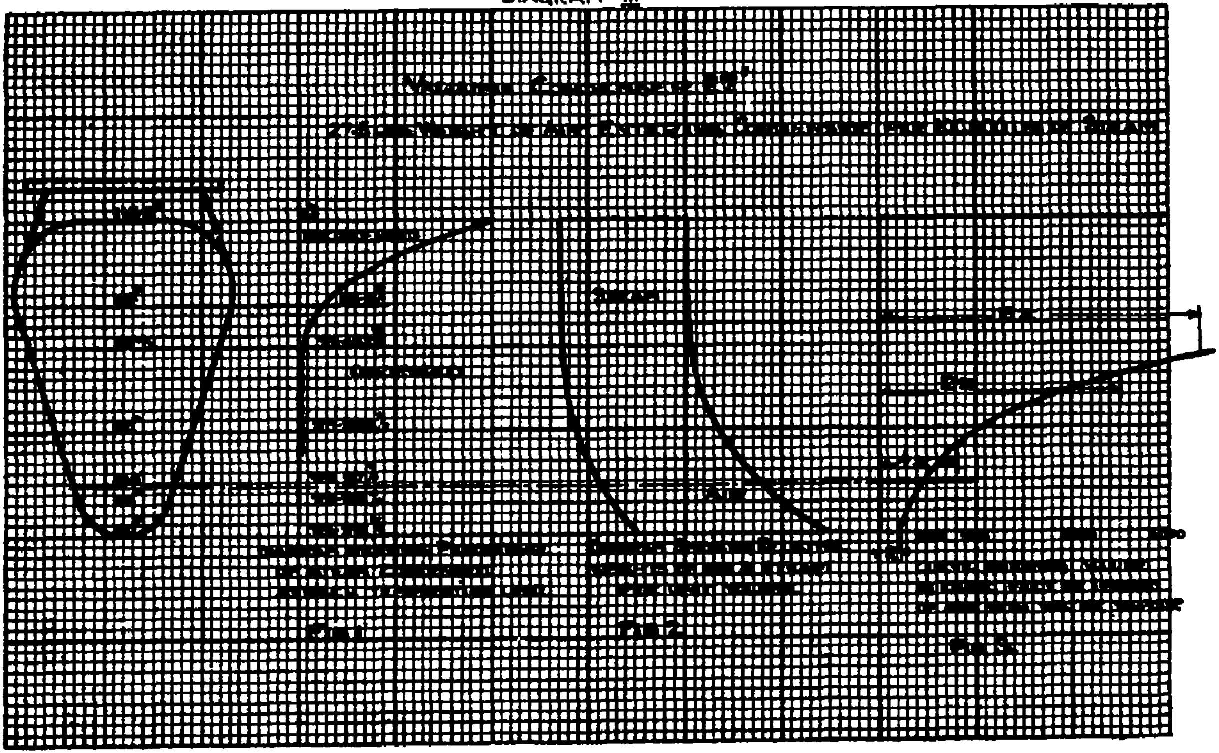
This condensation takes place at an exceedingly great disadvantage owing to the reduction in rate of heat transmission due to the relatively large weight of air present.

It follows that the extent of the inactive zone depends upon the volumetric capacity of the air removing apparatus, and that this dominates, to the exclusion of other factors, the average heat transmission co-efficient of the surface as a whole.

EFFICIENCY OF THF INACTIVE ZONE.

The air vapour mixture being very rich in its air contents, the heat transmission coefficient will approximate to that of pure air which is a well-known heat insulator.

DIAGRAM III



The coefficient of heat transmission for air according to Josse* depends upon the speed of flow of the air and also upon the absolute pressure; but at a pressure of ·11 atmosphere (26¾ inch vacuum) it does not vary much with the speed of air, and in no case does it rise above 5 at this pressure. For comparison with this Josse gives 20,000 for pure steam.

Hence it will be seen that the inactive zone is extremely inefficient, and that the direction in which surface economy may be promoted is in the elimination of that portion of the tube surface where the lower order of heat transmission rate prevails.

Diagram III., Figs. 1, 2, 3, which represents conditions in a condenser working at 27 inches vacuum, shows the percentage of steam condensed by reduction in temperature when associated with air in the ratio of 1 of air to 3640 by weight of steam entering the condenser. This proportion is usually accepted as representing average practice.

Figure 1 shows the percentage of steam which is condensed, and it will be seen that for 1½° F. fall in temperature from the temperature of the incoming steam about 99·5 per cent. of the total steam is condensed.

The portion of the condenser above this level may be regarded as the active zone, and obviously, practically the whole of the heat transmission takes place above this level and corresponds with the temperature rise in the circulating water.

The density and composition of the mixture are shown by abscissae on Fig. 2, measurements to the left of the vertical representing weight of steam per unit volume of the mixture and those to the right representing weight of air per unit volume. As condensation of the steam proceeds, the relative weights of air and steam will change and the air being non-condensible becomes the chief constituent.

Figure 3 shows the volume in cubic feet per lb. of air saturated with water vapour at the pressure existing in the condenser at each of the temperature levels. This volume may be regarded as a measure of the necessary withdrawing capacity of the air pump, the volume at the last temperature being denoted by x in Fig. 3 and in the following comparison :—

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* Vide Engineering, December, 1908.

TABLE II.

Volume abstracted.	x	$4x$	$12x$	$16x$
Ratio $\frac{\text{Air}}{\text{Vapour}}$	$\frac{1}{\cdot 311} = 3{\cdot}23$	$\frac{1}{2\ 6} = {\cdot}385$	$\frac{1}{10} = {\cdot}1$	$\frac{1}{16} = {\cdot}0625$
Temperature necessary at condenser bottom	80°F.	108°F	112°F.	113°F.
Thermal Efficiency .	69 5	94	97 3	98·2

It will be observed from the above that if the volume $4x$ of air and vapour is extracted from the condenser the temperature at the condenser bottom will be 108° F., as compared with 80° F. for volume x.

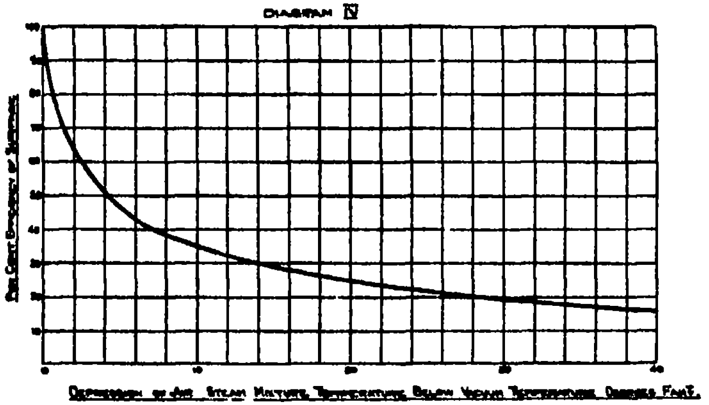
Since with the common air-pump the temperature of air pump discharge is practically the same as that at the bottom of the condenser, the latter may be taken in calculating the thermal efficiency as has been done for the fourth line in Table II.

THE DEPRESSION OF THE AIR PUMP SUCTION TEMPERATURE AS IN INDEX OF SURFACE EFFICIENCY.

Examination of the results of a number of tests on commercial condensers indicate a relation between the heat transfer coefficient and the difference between the steam temperature at the condenser inlet and the air temperature at the outlet.

The surface efficiency for a large number of commercial condensers are plotted against the difference between steam inlet and air suction temperatures on Diagram IV.

From the inclination of the curve it will be seen that the reduction of surface efficiency is more pronounced for a small increase in temperature depression when the initial depression is small than when it is large.



SECTION II.—APPLICATION OF STEAM JET TO RECIPROCATING AIR PUMP.

It has been shown that for the maintenance of any given vacuum with the ordinary proportions and arrangement of condenser working in direct communication with an engine driven reciprocating air pump a considerable portion of the tube surface must be employed in devaporising the non-condensible gases, so as to give the degree of concentration necessary to bring them within the volume fixed by the capacity of the air pump.

The consequent reduction in the amount of surface available for steam condensation renders it necessary to work with a large mean temperature difference between the steam temperature and the temperature of the circulating water.

The usual proportions adopted in practice with ordinary wet air pumps regarding tube surface and size of pumps render it impossible to secure the requisite degree of air concentration under conditions of high circulating water temperature or abnormal air leakage, except with a fall in vacuum.

In the system to be described these disadvantages are avoided by the adoption of an arrangement whereby the air pump is in direct communication with the condenser for the purpose of withdrawing the *condensate only*, together with the employment of a steam jet of much greater capacity than that of any practicable size of air pump for the extraction of the air and vapour.

A considerable proportion of the tube surface which in the ordinary arrangement of condenser would be devoted to the work of air concentration is thus made available for steam condensation, and permitting of the employment of a lower mean temperature difference between the steam and the cooling water.

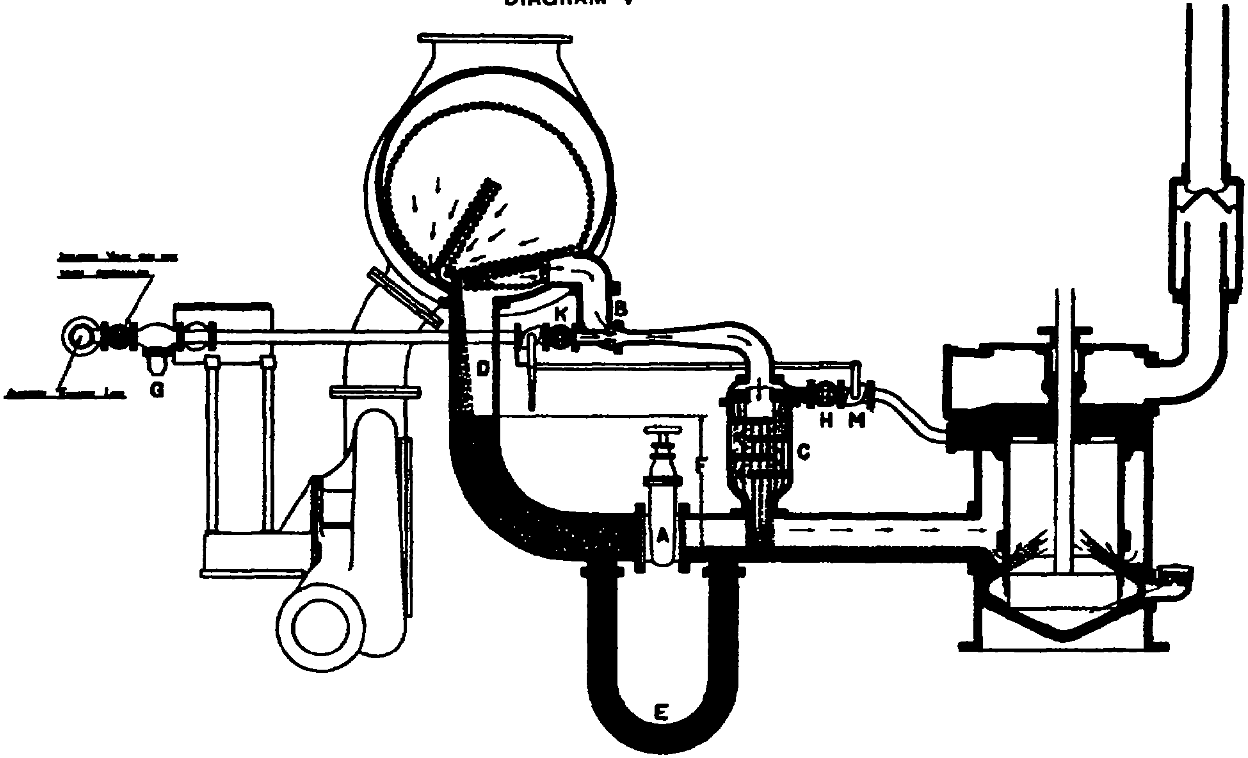
After withdrawal from the condenser by the steam jet the air is further devaporised in a small jet condenser supplied with cooling water from the hotwell and finally withdrawn by the air pump for expulsion to the atmosphere in the ordinary way.

The vacuum in the system will therefore principally depend upon the amount and temperature of the air passing to it, but since there is no direct communication between the air pump and condenser the vacuum in the main condenser is independent of that in the air pump system.

The reduction in the quantity of cooling water required at normal temperatures together with the flexibility of the steam jet as an air withdrawer provides the system with a reserve of capacity which enables an economical degree of vacuum to be maintained under conditions of high circulating water temperatures or abnormal air leakage.

The steam supply for the steam jet may be obtained by diverting a portion of the exhaust steam from the centrifugal circulating pump engine, and as the whole of the heat contained in this steam is conserved in the feed water the use of the steam jet does not entail any thermal loss.

DIAGRAM V



DETAILED DESCRIPTION OF AN INSTALLATION.

Referring to diagram v., A is the main communication sluice valve which is kept closed when the steam jet is in operation, thus isolating the air pump from direct communication with the condenser as regards air and vapour extraction.

Air and vapour are continuously withdrawn from the condenser by a steam jet at B and discharged into the small jet condenser C where the steam is condensed by direct contact with cooling water drawn from the hotwell.

The air is then extracted from the jet condenser C by the air pump and discharged to the atmosphere.

The condensate meanwhile passes down pipe ,D and is transferred to the air pump through the water sealing loop E, the balancing column of water F being maintained in pipe D by the difference in pressure between the air pump suction and the condenser.

It is possible (within the limits of the available height for this balancing column) to make the air in jet condenser C at any pressure in excess of that obtaining in the condenser which may be dictated by the prevailing conditions of operation.

The supply of steam for the jet is taken from the exhaust pipe of the centrifugal circulating pump engine, the remainder of the exhaust steam being passed into the auxiliary line through the spring loaded valve G whereby a constant back pressure of 10 lbs. is maintained at the circulating pump quite independent of any fluctuation of pressure of the auxiliary exhaust steam.

The chief objects of the above method of operation are:—

(*a*) To appreciably reduce the amount of tube surface, by eliminating the inactive zone.

(*b*) To relieve the condenser of the maximum of vapour consistent with the heat absorbing capacity of the water sprayed into the secondary condenser.

(*c*) To bring the temperature of the condensate and the temperature corresponding to the vacuum at the point of withdrawal to approximate equality under working conditions.

The temperature of condensate cannot exceed the temperature corresponding to the vacuum, but provided the disposition of tube surface is well considered that the condenser of such design to facilitate air concentration and the air extracting apparatus of sufficient volumetric capacity, it is possible for the condensate to be within 1° F. of the vacuum temperature.

(*d*) To withdraw from the condenser a large weight of vapour per lb. of air, the heat in this vapour to be absorbed by the feed water instead of being transferred to the circulating water and wasted.

ANALYSIS OF RESULTS SHOWING COMPARISON BETWEEN WITHDRAWING CAPACITY OF A STEAM JET AND RECIPROCATING AIR PUMP.

The author has carried out a large number of experiments for the purpose of comparing the relative air-withdrawing capacities of steam jets and air-pumps. In presenting here the results of some of these experiments, the effective volume of the reciprocating pump per stroke is taken as the volume of bucket displacement less the volume occupied by the condensate. For comparing the performance of the steam jet with the reciprocating pump, an effective efficiency has been adopted. This is defined as

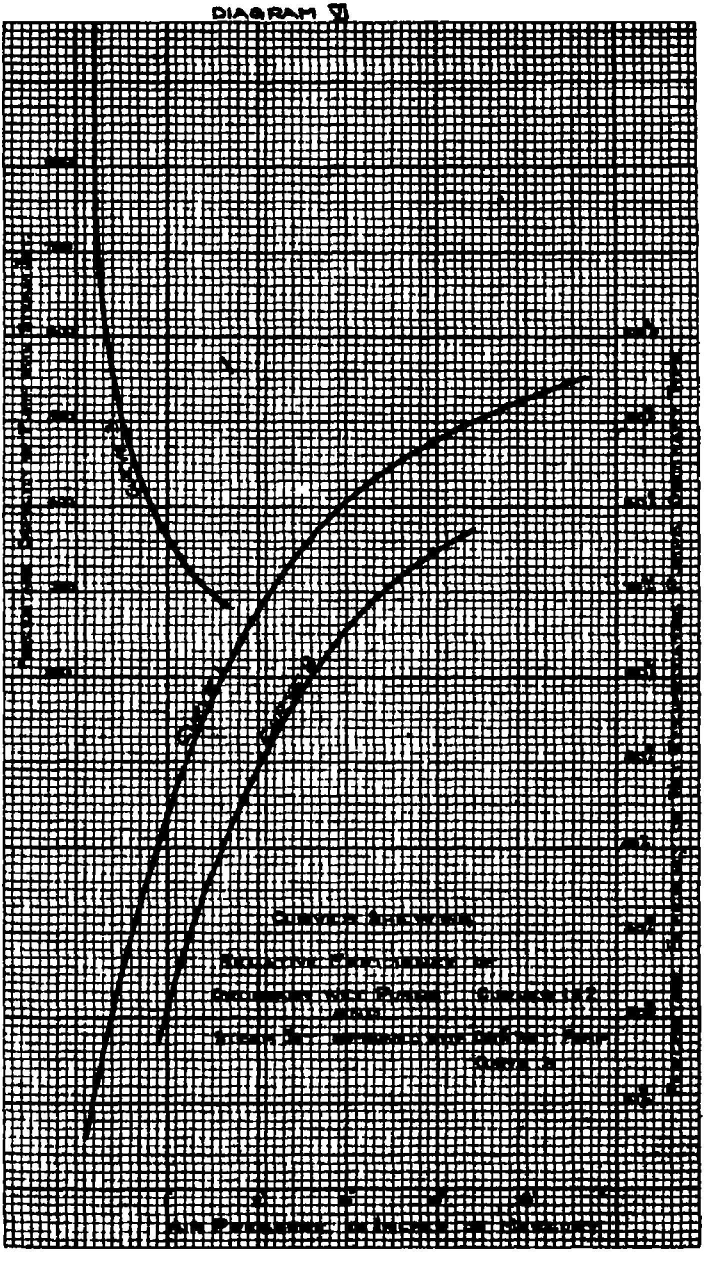
$$\frac{\text{Vol. withdrawn by steam jet per unit time.}}{\text{Displacement Vol. of air pump per unit time.}}$$

For the sizes of steam nozzles experimented with, the effective efficiencies at any air pressure were practically constant; also the volumetric efficiencies of this type of reciprocating air pump are practically independent of their size. The results of the tests can therefore be exhibited as in Diagram VI.

On this diagram the efficiency is plotted against the air pressure.

The inclination of the efficiency curves for the pump is directly opposite to the efficiency curve for the steam jet, the volumetric efficiency of the steam jet increasing when extracting the air at the lesser pressure, whilst

DIAGRAM VI


CURVES SHOWING
RELATIVE EFFICIENCY OF
ORDINARY WET PUMPS CURVES 1 & 2
AND
CURVE 3
CURVE 1
CURVE 2
AIR PRESSURE IN INCHES OF MERCURY

the volumetric efficiency of the wet air pump rapidly decreases.

These curves show the range of pressure in which either method of air extraction may be used to advantage.

It is evident that the steam jet should be used for the purpose of extracting air at extremely low density, and the reciprocating pump for a higher density.

The following (see Diagram VI.) indicates what may be done in practice, the steam jet extracting air and vapour at an air pressure of ·2 inches absolute, compressing the mixture to an air pressure of 4·36 inches absolute, and discharging into the jet condenser.

The reciprocating pump withdraws the air from the jet condenser at 4 36 inches absolute and discharges to atmospheric pressure. By this means the highest portion of the efficiency curves of both air extracting devices is made use of, the steam jet working at 800 per cent. effective efficiency, and the reciprocating air pump working at 90 per cent. efficiency.

It will be observed from the diagram that to produce ·2 inch absolute pressure of the air by means of the reciprocating pump, the pump would be working at 10 per cent, volumetric efficiency whilst the steam jet may be employed at 800 per cent. volumetric efficiency at this air pressure. This means that the steam jet is actually removing 80 times that volume of air and vapour from the condenser which the reciprocating pump could remove, or conversely if the steam jet were replaced by a reciprocating pump, the capacity of the pump must be increased 80 times.

SECTION III.—PRACTICAL APPLICATION OF THIS METHOD IN CARGO VESSEL.

COMPARISON OF PERFORMANCE OF CONDENSING PLANT FITTED WITH COMBINED STEAM JET AND RECIPROCATING AIR PUMP SYSTEM AND CONDENSING PLANTS OF ORDINARY DESIGN AS INSTALLED IN CARGO VESSELS.

The results given in Table III are obtained from an ordinary cargo vessel engine, the condenser having 1¼ sq.

ft. of cooling surface per I.H.P. and the air pump capacity being $\frac{1}{18}$ that of the low pressure cylinder, the condenser and air pump being of ordinary design.

The results given in Tables IV and V are obtained from an engine of similar general design and power. The condenser of this engine has 1 sq. ft. of cooling surface per I.H.P. and is fitted with the steam jet and reciprocating air pump system similar to that shown on Diagram V.

Results from ordinary cargo vessel fitted with ordinary wet air pump (Edwards type):—

TABLE III.

Revolutions of Engines per min.	Boiler pressure, lbs. sq. in. G	Vacuum by Gauge (ins.)	Temperatures °F				Vacuum corresponding to Air Pump Discharge.	Temperature corresponding to Vacuum.	Vacuum Efficiency per cent	Thermal Efficiency per cent	Surface Efficiency per cent
			Condenser Top	Air Pump Suction and Discharge	Circulating Water Inlet	Circulating Water Outlet					
77	179	27 8"	101	70°	43°	73°	29·26	104°	95·0	67·4	70·0
77	184	28·2	97	70	,,	75	29·26	97	96·5	72·3	77·3
77	180	27·8	103	70	,,	73	29 26	104	95·0	67·4	70·0
77	179	28·2	96	66	,,	70	29·36	97	96·0	68·1	72 1
78	182	28	96	66	,,	72	29 36	101	95·5	65·4	71·3
77 5	176	27 7	100	66	,,	70	29 36	106	94 5	62·3	66·1
78	182	28	96	65	,,	71	29 38	101	95·5	64·5	70·5
	Average		98 5	67 5							

Results from ordinary cargo vessels fitted with combined reciprocating air pump and steam jet:—

Indicated horse power of main engines = 2,300.

Cooling surface in condenser = 2,300 square feet.

Air pump (single barrel) = 21 inches × 24 inches.

Cylinders $\frac{25 \times 41 \times 68}{45}$

Ratio of air pump capacity to L.P. cylinder capacity $= \frac{1}{19{\cdot}7}$

TABLE IV.

Date	Revolutions of Engines, per minute.	Vacuum in Condenser inches, Hg	Vacuum in Jet Condenser C inches, Hg	Temperatures °F					Vacuum corresponding to Air Pump Discharge Temperature	Temperature corresponding to Vacuum in Condenser	Vacuum Efficiency, per cent.	Thermal Efficiency, per cent.	Surface Efficiency, per cent.
				Condenser Top	Air Pump Suction & Discharge	Circ'lating Water Inlet	Circ'lating Water Outlet	Condenser Bottom					
July 28,'18	82	25½	24	134	135	70	120	126	24·84	130	102·5	104	92·5
Aug 9, '18	65	26	24½	122	130	70	109	120	25 48	126	102·0	103·2	86·5
Aug. 10	64	25	24½	137	132	64	106	125	25·24	134	99·5	98 5	79·5
Aug 12	67	25½	24½	132	128	62	105	128	25·7	130	99·5	98·5	81·0
Aug. 13	63	26	24½	125	126	60	105	125	25·9	126	100·1	100	83·5
Aug. 14	72	25½	25	135	130	58	107	125	25·48	130	100	100	82·5
Aug. 15	78	25½	24½	132	135	56	115	122	24 84	130	102·7	104	88·5
	Average			131°				126°					

TABLE V.

Date	Revolutions of Engines per minute	Vacuum in Condenser C, inches Hg	Vacuum in Jet Condenser inches Hg	Temperatures °F					Vacuum corresponding to Air Pump Discharge Temperature.	Temperature corresponding to Vacuum in Condenser.	Vacuum Efficiency per cent.	Thermal Efficiency per cent.	Surface Efficiency.
				Condenser Top.	Air Pump Suction and Discharge	Circ'lating Water Inlet	Circ'lating Water Outlet	Condenser Bottom.					
an. 27,'19	61	27.4	24.75	110	130	54	96	108	25·48	110	107·5	118	87
an. 23	63	26.9	24.5	116	133	54	102	114	25·11	116	107·5	114·5	88
an. 24	64	27.2	24.5	113	132	56	100	112	25·24	113	107·5	116·5	88·
an. 25	64	26.75	24.25	118	134	60	106	116	24 98	118	107·5	113·5	90·
an. 26	65	26.6	24.0	120	134	64	108	118	24·98	120	106·5	111·5	90·
an. 27	64	26.75	24.0	118	134	66	106	116	24·98	118	107·5	113·5	90·
an. 28	66	27.4	24.5	110	132	68	102	109	25·24	110	104·5	120·0	93·
an. 30	65	27.4	24.0	110	132	70	100	109	25·24	110	104·5	120·0	91·
an. 31	64	27.5	24.5	109	136	70	100	107	24·71	109	111·0	125·0	92·
		Average		113·6				112·1					

The results shown in Tables III, IV, and V are taken from sister ships by the chief engineer of each respective ship. It will be noted that the data has been taken on successive days during a voyage, this accounting for the variation in the sea temperature, *i.e.*, the inlet circulating water temperature.

It is considered that the " surface efficiency " defined as the ratio

$$\frac{\text{Temperature of circulating water outlet,}}{\text{Temperature corresponding to condenser vacuum,}}$$

is the real measure of performance

The average fall in temperature from the top to the bottom of the condenser is only 5° F and 1 5° F (Tables IV and V) with the combined steam jet and reciprocating pump system, whilst with the ordinary system the fall in temperature is 31° (Table III)

THE CONCEPT OF BEHAVIOUR FROM THE STANDPOINT OF BIOLOGY.

By F W FLATTELY

[Read December, 1920]

The tendency of the present-day biologist to study animals* in relation to their normal environment, and not as isolated units, has greatly influenced his attitude towards the problem offered by their activities. It has led him, in particular, to recognize the importance of environmental factors in governing animal actions and reactions, and hence to the conclusion that much can be learnt about them by the employment of purely objective methods, without reference to considerations of "mind," "motive," "consciousness," and the like.

In addition to the influence of the ecological method of inquiry, the extremes to which anthropomorphic interpretations were so often pushed, and the complete disregard of the profound differences in the nervous organization of higher and lower forms, also helped to bring about a reaction. This is, however, by no means the first time that a revolt from the older viewpoint has occurred. Thus, in France, the extreme anthropomorphism of Montaigne may be said to have provoked the pronouncedly mechanistic theories of Descartes. Again, the outbreak of anthropomorphism which followed, curiously enough, the careful and valuable observations of Reaumur, led to the appearance of an opponent in the person of Lamarck who questioned the statement that the functions of the brain are of a different order from those of the other organs of the body, and saw in the term *esprit*, for instance, merely a device to evade difficulties, which it was found impossible, owing to ignorance, to surmount legitimately. In his stand against anthropomorphism, and in his objection to terms which have their counterpart in expressions such as " psychic " and the like Lamarck may be regarded as the forerunner of the modern behaviourist (Bohn, 1911).

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* Since no one doubts the adequacy of the objective method to supply an explanation of plant behaviour a discussion of this branch of the subject is unnecessary to our present purpose

The term " behaviour " would seem to owe its inception in biology to nothing more than the reluctance felt by a certain class of workers to make use of words which have a metaphysical flavour. It would now, however, appear to have acquired a more precise significance, since a school of " behaviourists," headed by Prof. J. B. Watson, has arisen with the intention of applying to the study of man's reactions the same objective methods which have been employed by the biologist.

The task that has been set the writer is that of discussing the concept of behaviour from the biological point of view Unfortunately, however, biology or, at any rate, the biologist has no precise viewpoint. Biology numbers in its ranks workers, such as Jacques Loeb and Hans Driesch, who stand at opposite poles in this matter. Perhaps it would be more correct to regard the one as a physiologist and the other as a philosopher, but it is certain that they would consider themselves (and rightly so) biologists. The writer, therefore, cannot claim to state the views of biology as a whole, for the term is far too comprehensive, but merely those of that section which regards the methods of exact science as most likely to shed real light on the problem of animal behaviour, and which is of Loeb's opinion (1918) that since we know nothing of the sensations of the lower animals, at any rate, and are quite incapable of measuring them, there is at present no place for them in science.

By emphasizing the necessity for employing the methods of exact science, the behaviourist does not mean to rule out speculation, but he demands that the speculations should be formulated in such terms as to allow of their being tested by the methods of experimental science. This is a point rightly emphasised by Prof. Watson.

At this point probably the best method of enlisting support for the behaviourist will be to bring forward one or two examples of what the objective method has already achieved in the field of animal conduct. Before doing so, however, we must draw attention to a very important divergence of opinion among those biologists who would probably also call themselves behaviourists. This has *its origin in the fact pointed out by Loeb (op. cit.) that many* biologists still cling to the view, a legacy from Aristotle, that an animal moves only for a purpose: either to look for food or to find a mate or to undertake something likely to secure the survival either of the individual, or of the race. Here again, the writer shares Loeb's opinion that "science began when Galileo overthrew the Aristotelean mode of thought and introduced the method of quantitative experiments which leads to mathematical laws free from the metaphysical conception of purpose," and that "the analysis of animal conduct only becomes scientific in so far as it drops the question of purpose and reduces the reaction of animals to quantitative laws" (Loeb *op. cit.*).

From this point of view, the drawback to Jenning's (1906) "trial and error" theory, which has acquired a very considerable vogue, is that it is based essentially on the purposive conception of behaviour. The merit of Loeb, on the other hand, is to have avoided this bias and to have attacked the subject of behaviour with an open mind. As a result he has been able to formulate the first theory of animal conduct capable of being tested experimentally, viz., the tropism theory.

A tropism—the term has long been familiar to plant physiologists—is an obligatory response under the direct compulsion of an external stimulus, as when a moth flies into the flame of a candle. The stimulus need not be provided by light; similar reactions occur in relation to gravity (*geotropism*), contact (*stereotropism*), chemical substances (*chemotropism*) and so on. The explanation of this type of behaviour is based on the symmetrical structure of the animal, symmetrical not only as regards structure but physiologically as well "inasmuch as under normal conditions the chemical constitution and the velocity of chemical reactions are the same for symmetrical elements of the body surface, *e.g.*, the sense organs" (Loeb *op. cit.*). When a sense organ of one side is stimulated and not the corresponding one of the other, as when light strikes one of the eyes, the physiological symmetry of the brain is disturbed and this disturbance being communicated to the muscles of the more illumined side they are thrown into a state of increased tension. The effect of this is to cause the animal to turn head and body until both eyes are equally illuminated when, the physiological symmetry being restored, the animal proceeds in a straight line towards the source of light. It is inaccurate to speak of a tropism as a reflex act. Reflex movements concern part of the body only, moreover they depend upon a particular arrangement of nervous units.

An important feature of tropisms is that they may be modified or even completely reversed by changes in the external medium. Thus, it is sufficient to add a little CO_2 to the water containing certain negatively phototropic Crustacea (species of *Daphnia*) to cause these animals at once to turn towards the light.

Now, this fact, according to Loeb, is capable of shedding new light upon the type of behaviour known as instinctive, and if such is the case then this distinguished worker deserves our thanks, for it is notorious that while biologists and others have spent a considerable amount of time in extolling the wonders of "instinct," they have apparently thought it either useless or beyond their powers to subject them to scientific analysis.

The view put forward by Loeb (*op. cit.*) is that the theory of tropisms is at the same time the theory of instincts if one takes into consideration the part played by chemical substances in modifying the tropistic responses. Reference has been made to the reversal of the behaviour towards light of certain freshwater Crustacea by CO_2. Now, it is well known that the body itself produces from time to time chemical susbtances known as hormones, and these may influence responses in just the same way as does the CO_2 in the case of the Crustacea, since, as Loeb points out, it makes no difference whether such substances as acid are introduced into the blood from within or without the body. Let us borrow a favourite example of Loeb's. The butterfly *Porthesia chrysorrhœa* lays its eggs upon a shrub. The larvæ hatch out in Autumn and hibernate on the shrub, usually close to the ground. In Spring the caterpillars leave their self-constructed nest and crawl up the stem to feed on the young plant buds. Should they crawl downwards they would starve, but they never do crawl downwards. What is the cause of this remarkable behaviour? According to Loeb, the upward movement of the caterpillar is due solely to the influence of light upon the physiological processes taking place within the body. These processes, acting in relation with the symmetrical structure of the animal, bring about a movement directly towards the light. In other words, the larvæ on becoming active after their winter rest are positively heliotropic.

As soon as the caterpillars have eaten the few young shoots at the top of the plant, they at once turn downward. The upward tendency which at first saved the animal's life would now, if persisted in, lead to its downfall. What brings about this remarkable change of behaviour? Simply the fact that the process of feeding has effected changes in metabolism which result in the animal becoming indifferent to light; these chemical changes, in fact, abolish the heliotropism just as the CO_2 arouses positive heliotropism in the case of the small freshwater Crustacea.

Experimental confirmation of the above facts may be obtained by placing starved and fed caterpillars of this species in separate test-tubes and submitting them to the same source of light. The unfed caterpillars will make their way towards the light and stay there while the fed ones remain completely indifferent.

In connection with this interpretation of instinctive behaviour it is interesting to remark on the view which is now beginning to be held, that the main factor concerned in the spawning migrations of fishes, such as the salmon, is a chemical substance or hormone. In fact, a recent scheme of Professor Meek's for the investigation of the salmon of the Coquet provides for the possible isolation of such a hormone. It is thus becoming evident that the fact that an action or series of actions is " instinctive " need not prevent our attempting its analysis by objective methods

In the course of an informal conversation upon the subject of behaviour with Prof. Hoernlé, the latter asked the writer whether or not he thought a higher animal such as a cat or a dog capable of " learning " and what place would the term " learning " take in a scheme of objective psychology. The writer replied that he thought it certain that animals such as those mentioned were capable of learning, and in precisely the same sense that we interpret that process in ourselves. But it is evident on the other hand that the term " learning " requires to be employed with very considerable care It is not, strictly speaking, a scientific term: we have very little conception of the mechanism of learning; we know, for instance, that in ourselves and in the highest types of animals it is associated in some way with the cerebral hemispheres. But how far is it possible to be sure that what we call learning in man and in the highest animals and what has been called learning in forms such as anemones, sponges, and even Protozoa belong to the same order of phenomena? When a piece of meat is placed upon the tentacles of a sea-anemone several times in succession, the tentacles on each occasion give a characteristic enveloping reaction. If, now, a piece of blotting paper is substituted for the meat the tentacles at once react in the same way. But if one continues to offer the anemone blotting paper instead of meat the tentacles eventually cease to give the reaction. Can the anemone now be said to have learnt?

When a muscle is stimulated several times in succession the effect of the second or third or later stimulation may be greater than that of the first, but gradually the response weakens and eventually it becomes zero. In this case no one feels the necessity of giving other than a physico-chemical explanation, and in fact such a one is soon found in the varying degree of hydrogen-ion concentration. But may not the behaviour of the anemone be due to analogous causes, and is it not the duty of science to exhaust such a possibility before taking refuge in transcendentalism?

Leob thinks that we can speak of learning only in such organisms in which the existence has been proved of what he terms "associative memory," and this theory has been developed by Bohn (1909-1911). The term memory is likely to prove equivocal, but the business of a behaviourist is to make himself understood, and it is not his fault if the whole of language is saturated with human meaning. By "associative memory" is meant "that mechanism (of which we are so far ignorant) by which a stimulus produces not only the direct effects determined by its nature, but also the effects of entirely different stimuli which at some former period by chance attacked the organism at the same time with the given stimulus." Everyone has experienced the power of a particular odour, for instance, to call up with great vividness a whole number of circumstances which impressed him at the same time as the odour.

Experimental evidence has been obtained of the existence of "associative memory" in certain species among vertebrates, insects, crustacea and cephalopods, but so far experiments have not demonstrated this phenomenon in worms, starfishes, sea-urchins, medusæ, hydroids or infusorians[*] (Loeb, *op. cit.*). In particular, it should be noted that the appearance for the first time, in the higher Crustacea, of a perfected visual (image-forming) organ, meant a great addition to the number and variety of

[*] In order to obtain an insight into the methods by which the strength of such associations may be measured, reference should be made to an account of Pavlov's salivation experiments. The original papers are in Russian but a summary will be found in Morgulis (see bibliography)

elements capable of entering into combination with one another to bring about a particular reaction or series of reactions. The fact that such combinations are numerous, complex and flexible (capable of being modified in a new environment) means that the behaviour tends to become less predictable and more "intelligent."

The same objections that have been urged against a too ready employment of the term "learning" apply with even greater force to the term "choice." Crabs of the genus *Maia*, which have the habit of covering their carapaces with red, green and brown seaweeds, etc., when placed in an aquarium where the illumination is of different colours will make their way to that portion where the colour of the light best matches that of the seaweed on their backs, and where they are in consequence least conspicuous.

In a case such as this the justification for endowing the animal with the power of choice is, even on the most generous view, so exceedingly slender as to make it a duty on the part of the observer first to exhaust all the means at his command of obtaining an objective explanation before so doing.

The view which the writer is endeavouring to put forward is that of the scientist who naturally wishes to apply to the problem of animal conduct the same objective and essentially quantitative methods which he has employed so successfully elsewhere. The behaviourist is in revolt against words which are likely to serve as a refuge from ignorance and against explanations of a nature which precludes their being tested experimentally. In the vocabulary of the psychologist there are words with a capacity for cloaking difficulties which can only be described as remarkable. Such a word, for instance, is "instinct." Of course, we should all know that "instinct" is merely a convenient term to describe an inherited capacity to perform a complicated action accurately at the first attempt, and without apprenticeship. Nevertheless, the fact remains that by hundreds of thousands of human beings the term instinct is accepted as providing an adequate explanation of all kinds of striking forms of behaviour, such as the migrations of birds and of fishes, the comb-building of bees and wasps and so on. It cannot be made too clear that the word "instinct" really explains nothing but is merely descriptive of a certain class of behaviour. Even as a descriptive term it is often misapplied.

The term "purposive" has perhaps more *raison d'être* than many others, but is hardly less dangerous. It is not denied that a very great many activities of both higher and lower animals appear to be directed towards a particular end, but is it really a satisfactory explanation of these activities to say that they are "purposive?" Presumably, when a biologist calls an animal action or structure purposive he is, more often than not, at the same time tacitly asserting his belief that it has been developed under the influence of Natural Selection. If this is so, then his explanation is a degree more satisfactory than that of the Aristotelian pure and simple, for he is describing, or at least delimiting, the factors which he supposes to have produced the behaviour or structure under consideration. Furthermore, they are factors which can be investigated—not metaphysical abstractions.

While admitting so much, however, the writer still holds that the teleological argument is somewhat of a nuisance in biology. It seems to have its reflection; for instance, in a too facile interpretation of the theories of Darwin; in an insufficient scepticism; in the readiness, for example, to interpret a structure or a piece of behaviour as adaptive, without experimental proof. Is it because the basis of our present human society is so pre-eminently utilitarian that an explanation based on "purpose" has such an irresistible appeal? One of the questions which the ordinary layman puts most often to the biologist is what is the *use* of such and such an animal? "What is the use of a barnacle?" I confess that this is a question which I am puzzled to answer. I can sketch the position of the barnacle in the "web of life," indicating its food and its enemies, but this is not what was wanted. Perhaps the best retort is another question: " What do you consider to be the use of man? " The truth of the matter is that the form of the question implies a wrong outlook on life, one which it is the duty of the biologist to correct.

Like all those who disturb long-held beliefs, the behaviourist is the victim of much misunderstanding and misrepresentation. His attitude towards most of the dogmas of classical psychology is not one of denial but merely of agnosticism. It is impossible to deny that before the advent of the behaviourist there was no science of animal conduct. Human and animal psychology were one, since all animals possessed minds and were actuated by whatever " motives " and " emotions " the human observer chose to endow them with. Biology is under a debt to the behaviourist for having pointed the way to impersonal and more truly scientific methods of analysing and interpreting animal conduct.

SHORT BIBLIOGRAPHY

The amount of literature dealing with the subject of animal conduct is considerable The following list contains references only to works which have a direct bearing on the subject matter of the foregoing article In addition to one or two of the most important books of a general character, a few papers are included which should serve to give the reader an insight into the methods of the behaviourist

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THE CONCEPT OF BEHAVIOUR FROM THE STANDPOINT OF PSYCHOLOGY

BY ARTHUR ROBINSON D C L

[Read December 1920]

My task would be all the easier if Behaviourists had all displayed the same scrupulousness in the use of terms which I find in Mr Flattely's paper If the terms ' learning," " memory, ' " choice " are not to bear their ordinary psychological sense it would be infinitely wiser to discard them, and in their strict sense the Behaviourist is not entitled to use them, nor does he need them unless it is to escape unconsciously from the limits of his initial abstractions However I must leave it to the biologist to argue the case between the notions of behaviour and trial and-error in his own field The obvious first question for the psychologist is whether mind means anything in man We have then to consider, (*a*) the main theses of Behaviourist psychology (*b*) its claim to dispossess the traditional view, and meet the problems which must be considered characteristic of any enquiry which can fairly be called psychological

(*a*) There are several types of Behaviourism According to some, behaviour is only a portion of the field of psychology, according to others the whole of it Further differences arise as it is held necessary or unnecessary to study the matter from a philosophic standpoint, and give a place to the findings of psychology in a reasonably coherent theory of things so far as we know them—an enterprise on which, I suspect all of us do in fact venture, some wittingly, others unwittingly Our present discussion will be limited to Scientific Behaviourism, according to which the task of psychology is the prediction and control of behaviour—just that and nothing more My account of this position is taken from two books by Professor John B

Watson,* in which his fundamental principles are stated with admirable clearness and precision. And the upshot is this.

The subject matter of Psychology is "*environmental adjustment;* what man can do apart from his training; what he can be trained to do, and what the best methods for training are; and finally, how, when the varied systems of instincts and habits have sufficiently developed, we can arrange the conditions for calling out appropriate action upon demand." (P., p. 9). "In each adjustment there is always both a *response or act* and a *stimulus or situation* which calls out that response. Without going too far beyond our facts, it seems possible to say that the stimulus is always provided by the environment, external to the body, or by the movements of man's own muscles and the secretion of his glands; finally, that the responses always follow relatively immediately upon the presentation or incidence of the stimulus. These are really assumptions, but they seem to be basal ones for psychology. Before we finally accept or reject them we shall have to examine into both the nature of the stimulus or situation, and of the response. If we provisionally accept them we may say that the goal of psychological study is the *ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or on the other hand, given the response, it can specify the nature of the effective stimulus.*" (P., pp. 9-10). The terms *Stimulus* and *Response* are used in their physiological sense; *Situation* means a complex group of stimuli, *adjustment, response* and *reaction* are, with Professor Watson, practically equivalent terms (P., p. 12). Behaviour is therefore physico—chemical change, and is either *explicit* or *implicit.* Explicit behaviour "involves the larger musculature in a way plainly apparent to direct observation"; implicit behaviour involves "only the speech mechanisms (or the larger musculature in a minimal way; *e.g.*, bodily attitudes or sets). Where explicit behaviour is delayed (*i.e.*, when deliberation ensues) the intervening time between stimulus and response is given over to implicit behaviour (to "thought processes.") (B. p. 19).*

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* ***Behaviour, an Introduction to Comparative Psychology,* New York, 1914 (B); *Psychology from the Standpoint of the Behaviourist,* London, 1919 (P).**

Mental imagery is held to be practically non-existent, affective processes to be simply muscle and gland activity, and with all this the occupation of introspective psychology seems to vanish.

"Will there be left over in psychology a world of pure psychics, to use Yerkes' term? The plans which we most favour for psychology lead practically to the ignoring of consciousness in the sense in which that term is used by psychologists to-day. We have virtually denied that this realm of psychics is open to experimental investigation. We do not wish to go further into the problem because its future rests with the metaphysician. If you will grant the behaviourist the right to use consciousness in the same way that other natural scientists employ it—*i.e.*, without making consciousness a special object of observation—you have granted all that our thesis requires." (B. p. 26).

Thus, just as the behaviour of animals can be studied without reference to their "consciousness," so can the behaviour of man without reference to his. In a word, the student of human psychology must take up precisely the position which Mr. Flattely has sketched as appropriate to the biologist in the study of animal life.

(*b*) It appears that the Behaviourist and the "traditional" Psychologist are asking different questions, and that the former flouts the latter for his quite natural failure to solve a problem which he never attacked. We may entirely agree that the physico-chemical responses of the human organism form excellent subject-matter for a scientific enquiry. It does not therefore follow that there cannot be a science of minds. Yet we are told such an enquiry belongs to the metaphysician, and this we should have to accept with the same feelings as a promise of payment at the Greek Kalends, for plainly relegation of a question to philosophy and metaphysics means with the majority of Behaviourists perpetual banishment beyond hope of solution. Though this again does not follow. Plainly there can only be a science of minds if (*a*) there are minds, and (*b*) knowledge about them be possible. It may help to consider what would seem to be the facts in regard to these points. But as a preliminary it will be necessary to refuse to ignore the agent's awareness of his own mind-field; in other words, to have recourse to the consciousness of the agent, as well as that of the investigator. For Psychology differs from Behaviourism in this vital point, that for the psychological enquiry an essential question is. Of what is the agent conscious? This question the Behaviourist need never ask. Hence it is that Behaviourism should never claim to *replace* psychology.

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* Professor Watson now holds that thinking is not merely the action of speech-mechanisms. "A whole man thinks with his whole body in each and in every part." *British Journal of Psychology*, vol. XI, Pt. I p. 88

It may be objected that I have merely re-introduced introspection thinly disguised in a phrase. I do not like the term introspection and its controversial implications. All that is required for the present contention is that there should be conscious beings, and that they should be able to say of what they are conscious; and these facts nobody will deny. But where is there any serious proof that it is unscientific or irrelevant to the "prediction and control of human behaviour" to take into account the consciousness of the agent? Nothing mystical or metaphysical is claimed. On the contrary, it is maintained that *quâ* knowing there is no difference relevant to science between knowing that I have a headache and that I have a hat. To this extent therefore Psychology must be "introspective."

There need be nothing "metaphysical" in the notion of a mind, so far as such a notion is necessary for a Psychology which claims to be a science of mind. No one cares to deny the existence of consciousness in the sense of awareness, and in that sense only do I use the term, for as Professor Ward says, the "manifold ambiguities" of the term "consciousness" are something of a scandal. (Principles of Psychology, p. 21). Every conscious being is an experient and the unity of a mind is simply the unity of different modes of experience (sensing, perceiving, inferring, hoping, fearing, &c) in an individual whole. It is true that this unity is *sui generis* and cannot be exhibited as a spatial and physical system, but it is not "mystical," nor does it imply any "ghost-theory" as some Behaviourists most strangely suggest. It is implied in principle in the simple fact that there can be no inference unless both premises and conclusion fall within one and the same mind, or that there can be no disappointment unless the mind which feels it be the mind which had hoped.*

Such is the field of the traditional Psychology. It presents a genuine problem but Behaviourism does not touch it.

Behaviourism will be right in ignoring mental processes on either of two suppositions, (1) if there are no mental processes, (2) if, though mental processes exist, they make no difference. The first alternative can only be taken at the expense of asserting the bare identity of an emotion of fear, for example, with a bodily change involving the activity of muscle and gland, and saying that they are one and the same event. Such activities may be conditions of, or factors in, an emotional experience, but they cannot be the emotion. Nor can the emotion be merely the awareness of such bodily processes: men feared for ages before the action of the endocrine glands was known. It is only by a series of convulsive efforts that the Behaviourists can endeavour to ignore the specific qualitative difference between such experiences as fearing, hoping, &c., and their undoubted bodily concomitants. Witness the strange definition of curiosity as "investigatory behaviour." How can behaviour be "investigatory," unless the agent wants to know something? Similarly thinking cannot be resolved into "implicit behaviour," conclusions do not follow from premises according to physico-chemical laws. If the Behaviourist could overcome his dread of the introspective and the subjective, it would be interesting to hear from him what is implied in addressing arguments to those who differ from him. Are the arguments merely marks on paper or noises of a certain pattern and sequential order? And keeping within the abstractions of the theory, what would be the difference in the marks and noises of one converted to Behaviourism and of an unrepentant traditionalist? For, *ex hypothesi*, in each case the consciousness of the agent counts for nothing. This brings us to the second alternative, *viz.*: that though mental processes exist, they make no difference to behaviour. But this is simply epiphenomenalism—a position as repugnant to the Behaviourist as to most other people, and if anyone believes that consciousness is no use, it would be the height of ineptitude to reason with him, for the simple reason that, if it be true, he is not entitled to affirm either this or any other proposition.

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* See Stout's *Manual of Psychology*, Ch. iii, §§4, & 5.

In short, my position is that Behaviourism is not and cannot be Psychology; each tackles a different problem. Behaviourism has before it a perfectly definite scientific question the answer to which is not yet worked out, but the enquiry and its result bear undoubtedly on the findings of Psychology, which science has still a long way to go largely because other sciences have endeavoured to impose on it their own categories and methods.

THE CONCEPT OF BEHAVIOUR FROM THE STANDPOINT OF APPLIED PSYCHOLOGY

By GODFREY H THOMSON

[Read December 1920]

The interesting papers of Mr Flattely and Professor Robinson reach me at a time when the pressure of official duties forbids me to do more than express crudely the point of view which I feel somewhat keenly and which I would have liked had it been possible to expound with the care which its importance I believe warrants This point of view is that held by many of those who wish to use the teachings of psychology to enable them the better to influence their fellows to handle men to control classes of boys to manage workmen even to sell goods to the great British public

Of these I will confine myself to the class for which alone I am in some measure competent to speak those engaged in teaching children All whose duty it is to train teachers recognise that the problem is almost entirely a psychological one and in consequence lectures and exercises on psychology have for years been given in training colleges and in education departments of university colleges The position I wish to urge is put somewhat forcibly more forcibly than I would care to defend in its entirety when I say that it is my belief that this teaching of psychology in training colleges has been of use to teachers in exact proportion to the extent to which it has approached the Behaviourist attitude and in inverse proportion to the extent to which it has clung to the metaphysical side of psychology I repeat that I give this as an extreme statement of the case and one to which I shall myself in a later paragraph take exception but it is useful to have one's general position clearly put in its naked form at the outset

All who have studied the question will agree that there exist English textbooks of psychology which are among the best of their kind and are accurate and lucid statements of the truths of psychology from the point of view which may be called the classical or legitimate Many teachers have during their period of training studied these textbooks, and some of them have even become interested in the problems therein considered and contributed somewhat to the progress of the discipline But it would be difficult to find many or any who would give it as their honest opinion that such psychology has in the slightest degree helped them in their work of teaching One finds, indeed, among teachers the remarkable fact that they have studied psychology and still remember some of the technical language of the subject but find it of no use while on the other hand as successful teachers many of them clearly have a considerable body of psychological knowledge at command which they do not recognize as psychology and which they can only express in what is I claim essentially the language of Behaviourism even though it be impregnated with anthropomorphic terms The teacher knows that if a boy, or a whole class acts in such and such a way, then by making such and such changes in the situation the teacher can with frequent success control the actions of the class or the individual He becomes expert at making these adjustments and formulates usually some incomplete but practical theory to assist him in integrating his devices in his own mind Such theories are indeed often as I have said expressed in terms of the mind of " faculties ' which even the classic psychologists themselves reject and generally in the jargon of a psychology compounded of popular language and the technical terms of an old textbook But in spite of this the psychology of the average teacher, with which he works and in some cases succeeds, is I claim more Behaviourist than not

Moreover, I have repeatedly found that teachers who are impatient or derisive when orthodox psychology is concerned listen willingly and with interest to remarks taken direct from the writings of men of the Behaviourist shade of thought, although the arch-priests of the Behaviourist hierarchy are no doubt almost as repellent as are orthodox writers of textbooks Indeed they recognise at once that this psychology (if it is psychology at all as Professor Robinson would no doubt interject) has at any rate condescended to come down from the clouds and say in plain language what things one has to do or say in order *to persuade certain of our fellow mortals to act or speak in* a certain way, and this is essentially Behaviourism No doubt, if the new psychology went so far as to deny that in this process there was any question of consciousness or mind if it refused to take advantage of the many terms lying ready to hand which if challenged one has to admit do imply the existence of mind and consciousness, the teacher would feel again estranged But in fighting this particular and extreme brand of Behaviourism Professor Robinson is in fact missing the situation as I see it He is confining himself to holding up the bodyguard of the enemy's monarch oblivious of the fact that the thousands who form his main army are marching on and surrounding his position and that presently he will wake up to the truth that although he has checkmated the king the kingdom of Behaviourism has come into its own

The reasons why the teacher who is in practice I contend a Behaviourist would in most cases at once repudiate Behaviourism as expounded to him by Professor Robinson, are two both founded on the fact that the *exclusive* preoccupation with the Behaviourist point of view tends to produce in the student a mechanistic philosophy Now a mechanistic philosophy is, I venture to say repugnant even to those who hold it inasmuch as it wounds our *amour propre* by denying our freedom of will, and because it terrifies us by suggesting that there is nothing more in us than that which dies when the body is destroyed Those men who have in spite of this held an extreme form of mechanistic philosophy have done so because another force was still stronger the force of what they believed to be logical conviction. I think I am right in saying that no one who has not gone through a rigid course of scientific training, persisted in for years, can realize the way in which the logical argument in favour of mechanism appeals with almost overwhelming force to the mind of such a worker. I remember to have read somewhere of Huxley's retort to some clergyman who was opposing the view held by him, that he too would hold the same beliefs were he to submit himself to a three years' course of biological training. Our beliefs are indeed to an extent hardly realized by those who have always worked on the one side of the fence which separates natural science from the humanities, dependent on our environment and our customs. There, but for the grace of God, goes a philosopher, or a Behaviouristic biologist, might with equal reason be said, *mutatis mutandis*, by Flattely and Robinson of each other, might it not? With *equal* reason—and so also might the clergyman have retorted to Huxley that a three years' course of slumming with a Gospeller might change his views. And so although I think that psychology in order to be useful to teachers (and others who have to apply it) must consent in large measure to speak the language of Behaviourism, or at least must appeal as much as possible to what can obviously be seen and heard of the "thoughts" of our pupils, yet I do not wish to be, erroneously, associated with those who deny mind entirely. I am convinced that they only are provoked to do so by distrust of the remarkable power of splitting hairs and using vague words which is displayed, or so the scientist often thinks, in many philosophical writings—I trust Professor Robinson and Professor Hoernlé will pardon me, for truly the shaft is not aimed at them. So often the scientist who is drawn into a philosophical argument finds, in the twinkling of an eye and in the turning of a page, that the matter has got beyond his ken and indeed beyond all knowing, that it has been lifted on to a plane where, he is assured, words, being anthropomorphic things invented to describe mere appearances, cannot be used except as vague hints towards the transcendental argument which is progressing, that he may be excused for doggedly refusing to concede the first point The teacher wants to know how to deal say with a boy who is intelligent but sulky and on turning for help to his text-book of psychology finds only too often that even the most promising references in the index turn out to be devoted to such matters as distinguishing between the satisfaction of conation and its object or emphasising the fact that self-consciousness falls under the principle of continuity, truths not particularly helpful to him at the moment

On the other hand he will even though he is I claim, essentially a Behaviourist in his dealings with his pupils, be particularly repelled by the refusal of the Behaviourists proper to permit him to speak of *purpose* No word is more useful and no idea more important, in teaching or in training teachers than *purpose* The same post which this morning has brought me the manuscript of the two preceding papers of this symposium has brought me an article in MS on the Teaching of Mathematics, by a distinguished mathematician and experienced teacher and inspector of schools which opens with the sentence "The basis of all good workmanship is *purpose*" and I find myself agreeing entirely A recent and very welcome book from the pen of Professor T P Nunn on the fundamentals of education deals almost throughout with the concept of *purpose* My readers will realize from my appreciation of this that I do not march in the ranks of the out-and-out Behaviourists All that I have urged here is that a great deal of pruning with the Behaviourist bill hook would be of advantage to the health of the psychological tree of knowledge, and in particular would enable the applied psychologist to feel less acutely the present divorce between theory and practice

A PHILOSOPHER'S COMMENTS ON THE BEHAVIOUR OF BEHAVIOURISTS

By R F ALFRED HOERNLÉ

[Read December 19_0]

1 —My contribution to the symposium in which Mr Flattely, and Professors Robinson and Thomson have preceded me may be summed up thus Behaviour—yes, Behaviourism—no

I welcome the introduction of the term "behaviour" as a most valuable advance, as a return to the concrete from what I should call "abstractions" and what Mr Flattely and Professor Thomson call "metaphysical" and "transcendental" vapourings What I reject as "behaviourism" is the extreme theory which affirms that there is no such thing as "mind," even in human behaviour With Professor Thomson's "behaviourism" which concedes that children and teachers have minds I have no quarrel My disagreement with Professor Thomson begins when he connects even his own behaviourism in the name of science, with "mechanistic philosophy," and accuses those of us who find mechanism inadequate as a philosophy, of wounded *amour propre*, and of having God and immortality up our sleeves * Personally, I have no theological axe to grind in this argument My only concern is to save the mechanist from himself—to affirm the existence of the mechanist s own mind that mind which the mechanist himself uses in the very act of denying that he has any such thing

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* I hope Professor Hoernlé will *forgive* me if I exercise an editor's privilege by adding this footnote Since I fear that his words may convey to the reader the opposite impression I would like to say that I *myself* do not find mechanism adequate as a philosophy, that I myself *feel* a wounded *amour propre* at the idea of my actions and even my thoughts being predetermined and that I want to cling to God and immortality My difficulty the source of much mental pain to many scientists is simply that I am led to determinism whenever I try to conduct a strict argument with myself If Professor Hoernlé does not feel the dilemma or has solved it, I envy him, but do not understand him I think it is insoluble, and find my relief in this confession and a blind hope—or *faith*—that there is light *for* those who can see

I am glad to think that I have Mr Flattely on my side For Mr Flattely assures us that Loeb attacked the subject of behaviour with an *open mind* I agree that Loeb has a mind I do not agree that it is an open one Of course to plunge into the study of animal behaviour with the naive assumption that every animal is a miniature man and then to attribute to animals all sorts of purposes is reckless And to be made aware of such uncriticised assumptions is to have one s mind opened But Loeb s mind I submit is *closed* viz closed *against* purpose And note it is closed not because the *facts* positively exclude purpose but because of his ideal of *scientific method* excludes purpose For him science means as Mr Flattely says reducing the reactions of animals to quantitative laws Excellent ideal in itself but I am bound to point out (*a*) that there is nothing in the concept of a purposive action which excludes its having a strictly determinate quantitative side (*e g* when I voluntarily lift a heavy object the amount of muscular energy which I must put forth is a determinate function of the weight of the object and the height to which I lift it) and (*b*) that when you make up your mind to look for nothing but quantitative laws you will find nothing but quantitative laws Hence even if there were a purpose in every action of an animal the mechanist would by his whole programme and method be incapacitated from discovering it I am far from saying that all animal behaviour is purposive but I do say that mechanism is by definition committed to the enterprise of thinking in terms only of ' matter and motion ' and that for this very reason it cannot recognise anything else in the world But it is one thing to ignore mind even where it occurs because your method does not enable you to deal with it It is another thing to deny the existence of minds altogether. As well might a blind man deny that there is light for those who can see.

Loeb denies even of human beings that they have purposes. Professor Thomson insists that they have. In fact, he is a behaviourist precisely because he is thinking of the teacher's problem of controlling the purposive behaviour of children. Now, when one behaviourist rejects "mind" and "purpose" utterly, and the other acclaims them, and both do so on the ground of "science" and "mechanism," what, I ask you, is the poor philosopher, whom both abuse, to think of such behaviour?

2.—But enough of dialectics and debating points. It is evident that behaviourists themselves are in two minds about what "behaviour" is. Now, as a philosopher, I find the concept of "behaviour" far too valuable, not to come to its rescue to the best of my powers, when I find it thus maltreated by its scientific friends. Let me, then, define the problem as I see it.

The question for me is: Does any or all behaviour involve mind or consciousness? In other words, where, if anywhere, are we entitled to say that the behaviour of a living creature expresses what the creature feels, perceives. thinks, wills, etc.?

As a philosopher, having an open mind, *i.e.*, a mind open to every datum, hint, or clue, which my experience anywhere affords me, and refusing to be blinkered by any restrictions or taboos (even though they be disguised as "scientific method"), I seek my answer over the whole field of my experience. Science, on the other hand, takes, so far as I can see, one or other of two restricted lines. (1) It denies outright that there is any such thing as mind in the world, on the ground that, in last analysis, everything that exists can be exhaustively described in purely physico-chemical terms. This is the extreme mechanistic position, which, in the guise of the extreme type of behaviourism, is now invading psychology. Thus, J. B. Watson, the leader of this school, explicitly rejects the terms "mind," "consciousness," etc., from psychology, and expresses the hope that psychology conceived as the study of mind. will soon be as defunct as alchemy. (2) Or, more mildly, science allows that mind exists but denies that it can be dealt with by scientific methods.

Now, with this latter position a philosopher need have no quarrel at all. For, strictly, it is not a criticism of mind, but a self-criticism of science. It is a modest acknowledgment of the inherent limitation of scientific method. To say that by scientific methods we cannot find out anything about the mind, leaves it open to us to try other methods. But the danger is that we restrict "knowledge" to what we can find out by scientific methods, and then slip into saying that, because by such methods we can know nothing of mind, therefore there is nothing there to know.

But, further—and this is perhaps the most important point—behaviourists are recruited chiefly from two classes of scientists. They either, like Professor Thomson, have had a training in physics and chemistry, or else, they are biologists, studying by preference non-human animals, if not more or less primitive micro-organisms. Now the objects which physics and chemistry study have no minds, as a rule; or, if they have minds, as when a physical or chemical experiment is made on a living human being, their minds are ignored as irrelevant. Again, in biology, the further away from man we get in the animal kingdom, the more precarious and ambiguous undoubtedly is the evidence for deciding whether an animal has a mind. or what sort of mind it has.

Hardly less important is the fact that a creature's body and its movements in its environment are observable by the senses; observable, too, by any number of observers at once. The creature's mind, on the other hand, is not observable by outsiders. Add that we can, more or less, control the creature's movements experimentally, and you get a total situation in which, from a convergence of reasons, it is wholly intelligible even to a philosopher that scientists in these fields should try to get on without reference to mind.

More, we can understand, too, why these methods should be extended to the study of human beings; why scientists should try how much they can do with them even in the sphere of human behaviour where, after all, we are sure of mind, however much we may pretend that there is no such thing.

As we are about now to approach the point of disagreement, let us set down, first, what we are apparently agreed on. We agree:—

(i) That one of the chief tasks of science is to formulate quantitative laws wherever it can, and that there is nothing in conscious purposive behaviour which forbids this or makes it impossible.

(ii) That our quantities refer to the physical side of behaviour, not to the mind which expresses itself in that behaviour.

(iii) That even if our object is, not quantitative laws, as for Mr. Flattely, but simply control, as for Professor Thomson, we certainly develop a technique for experimenting upon human beings so as to elicit, by suitable stimuli, the kind of response we want.

(iv) That the teacher is fully aware that he is experimenting upon the children's minds, which are embodied minds, and within fairly wide limits, what the nature of their minds is, *i.e.*, what they are thinking, feeling, etc What is true of teacher and children is, in general, true of all intercourse of human beings with each other. For all our frequent misunderstandings, we still deal fundamentally with each other mind to mind.

(v) That human minds are not mysterious entities open only to introspection and describable only in "metaphysical" language, but that *they are expressed and thus made known, through behaviour.*

(vi) That the attempt to extend this concept of mind from human to non-human animals becomes increasingly precarious as we get further away from the human type, and that a point comes where analogy, in unskilful hands, becomes a snare rather than a help.

In short, whilst I grant heartily the scientific ideals of quantitative determination and of control and the difficulty of applying mental terms to the behaviour of lower animals, I ask scientists to grant me that each of us knows that he has a mind himself, that we know of each other that we have minds, that we can find out a good deal about each others' minds, and experiment upon them by stimulating one another's bodies and getting appropriate or inappropriate behaviour in return

3—What, then, is the difference between human and animal behaviour which compels us to acknowledge minds in man whilst permitting us to ignore them in animals?

I suggest that the difference lies in the use of *speech* Speech is a kind of behaviour It conforms to the pattern of all behaviour it is a specific response, in terms of articulatory movements of the speech organs, to some stimulus But speech is more than a series of noise-producing movements The noises are not mere noises incidental to movement like the roar of a waterfall, or the rustling of leaves in a breeze, or the clanking of the railway train They are expressive noises They are noises made for the sake of expressing, or conveying, something Recent analysis has distinguished two kinds of meaning in language When, perceiving a dog, I say There goes a dog," I *express* the fact that I see and I *state* what it is that I see In general I express (*ausdrücken*) my mental acts and I state (*aussagen*) what are the objects of those acts Of course I can make the act part of what I state, *e g*, "I see a dog" As Kant pointed out in another context Take any statement of fact and you can always preface it by 'I think" The points, then, on which I would insist are (1) that all language is expressive of mind and (2) that we have a special set of terms, a vocabulary of words for stating the occurrence of the most obvious kinds of mental acts which have forced themselves upon the attention of the human race We cannot in dealing with human beings either deny (1), or, admitting (2), refuse to use the language of "mind"

But why can we do so with animals? Partly, because no animal in *rerum natura* possesses articulate speech. But even more, because the vast majority of animals, especially those which are observed and experimented on in laboratories utter no noises at all. Where there is no cry of pain or fear, no snarl of rage, our commoner evidence for predicating feelings and emotions is lacking, and it may plausibly be called sentimental nonsense to endow the silent animal with any sort of consciousness. To ignore the expressiveness of the cries of the higher animals is not so easy, yet to save the theory it can be done because no animal can actually tell us in words that it feels or what it feels. Certainly, on the mechanical, or automaton, theory of animals, no animal's cries can possibly express feelings, *for* it has not any feelings to express. The noise which a dog makes when you kick it will be as inexpressive as the noise which a tin can make when you kick it. It was no mere accident that the automaton theory of animals, when it first became popular, unloosened an orgy of vivisection. No tender consciences were stung by the victim's cries which were but the noises of a machine being taken to pieces. Our forefathers at least took their theory seriously. When they said "machine" they meant machine, and behaved accordingly.

The scientists of our humanitarian age try hard to keep up the machine-language for all animals, and to shut their eyes to the fact that their behaviour, especially in its emotional, æsthetic, *moral* aspects, and towards the higher and particularly the domesticated animals implies quite a different theory. A biologist talks one theory in his books, he lives another when, *e.g.*, he enjoys the affection of his dog. It is part of a philosopher's business to be sensitive to inconsistencies of this sort, and to remind the biologist of the sanities of commonsense which finespun theory is so apt to forget.

At any rate, I venture to suggest that the methodological ignoring of mind is immensely facilitated by studying non-human animals rather than human, micro-organisms rather than macro-organisms, creatures incapable of expressive cries rather than creatures with voice to utter cry and speech

If these suggestions are sound they will explain why the uncritical use of the mind language in the description of the behaviour of lower animals is open to grave objections It has often led to fantastic 'humanising' of animals But, surely, to refuse as Watson does, to use the mind language in describing the behaviour of human beings, is equally fantastic Watson is clear headed enough to insist on the full consequences of his position which involve the denial that speech-behaviour is, *par excellence* expressive of mind that language has meaning Here are his own words —

> 'I should like to say frankly and without combativeness that I have no sympathy with those psychologists and philosophers who try to introduce a concept of 'meaning' into behaviour At every point we would describe all of psychology in terms of what we see the organism doing The question of meaning is an abstraction a rationalisation and a speculation serving no useful scientific purpose From the bystander's or behaviourist's point of view the problem never arises We watch what the animal or human being is doing He means what he does His action is the meaning Hence, exhaust the concept of action and we have exhausted the concept of meaning "

In his denial of meaning I believe Watson to be as much wrong as in his denial of imagery In treating thinking as talking, and thus as part of bodily activity open to an observer, Watson very cleverly shelves the question of the meanings of all the characteristic terms of the mind-language He absolves himself from considering what it is in our experience that we express when we use such terms as feeling, thinking, willing It is worth noting in this connection that Watson, in saying that psychology is to frame its descriptions in "*terms of* what we see the organism doing," appears to take both "seeing" and "doing" with extreme literalness. I strongly suspect that he is forgetting, for the moment, that we can *hear* the sounds which animals and men utter, and hear, too, even when we understand nothing else, the emotions which sounds express. It is certainly striking that in his discussion of thinking Watson turns his and our attention almost exclusively to the muscular movements of the speech-organs, not to the sounds produced. Yet it is by the sounds which we hear, or the writing and print which stand for sounds, that we learn chiefly what other people are thinking or have thought, not by studying, however closely, the movements of their lips, tongue, and larynx. It may be hard to give an account of what meaning is, but we are familiar enough with it to know what we mean when we use the word. It is true that such phrases as "we can make our subjects think aloud and thereby can observe a large part of the process of thinking" (p. 39) do occur in Watson's text. But, Watson, I feel sure, has in mind neither that the subject is *expressing* what he thinks, nor that the observer "observes" the thinking by *understanding* what the words mean, and not merely by hearing them or watching the speech muscles. What it comes to is this: In denying all introspection and self-knowledge as "mystical" and "transcendental" and limiting the psychologist, even in the study of human beings, to the point of view of the outside observer exclusively, Watson fails to use all the evidence at his command. The terms of the mind-language owe their meaning to two sources of evidence. One source is our observation of the behaviour of others, including their speech-behaviour, the latter more particularly being interpreted by us as expressive of the others' mind. The second source of evidence, which makes this interpretation of others possible, is our direct acquaintance with our own minds and with the way we express our minds in action and speech. As things stand now, such a statement as:

"the dog is looking at the moon" is liable to be translated, in the name of science, into one or other of two formulæ, viz., either "the dog is responding to a visual stimulus" or "a visual sensation is occurring in the dog's stream of consciousness." The first formula, expanded, leads to physiology; the second, to traditional psychology talking in terms of inward states of mind. This bifurcation of body and mind can end only in psychophysical parallelism. Against this the theory of behaviour as *expressive* of mind, which I am here trying to advocate, is a protest. It is a return to a point of view both more concrete and, perhaps, more naive—at any rate, nearer to commonsense. Ask any scientist to give an account of what he is doing in pursuing his scientific work, and you will find him using mind-language all along the line He will tell you what he saw and heard; perhaps what he smelt, tasted, felt by touch; what he thought, inferred, suspected, concluded; what he hoped or feared; tried to do and perhaps failed; his joy at success; his efforts Mind-language all the way: and we know what the terms mean without any mysterious or mystical self-knowledge of the introspective sort, and also without previous observation of the behaviour of others. All these terms of the mind-language express *activities directly experienced in ourselves*, not observed first in others. They are not terms invented to record what we first note in others and then, by inference, apply to ourselves. They draw their meanings from that direct, unreflective self-knowledge which is the basis of our interpretation of other minds. "Fear," for example, is a word used to express a distinctive feeling which we experience. We know what "fear" means by feeling fear. The knowledge by personal experience of what it feels like to be afraid is, of course, developed by watching frightened fellow men or animals. But such watching of others alone could not supply the meaning which "fear" actually has for all who have felt it. A person who was wholly unacquainted with fear in himself would stand before the expressions of fear in others as before a riddle which he could not understand. He might set himself to study this strange behaviour, noticing the conditions under which it appears, and the consequences which it has, but he would never get beyond an *outsider's* or "bystander's" knowledge of fear. It may be said: "This is all we want in science." The answer is: "It is not all we *know*." To say it is all we want in science, is a plain confession that relevant evidence is ignored, because it does not fit into some programme of scientific method. Now science is justified in *abstracting* and *selecting* as much as it may find convenient. But we cannot both ignore evidence and also set up the claim that the result is all we really "know." In situations such as this it is the philosopher's business to be the guardian of *all* the evidence which experience puts at our disposal. And anyone whose mind is open to all the evidence will, if he reflects, refrain from any behaviour so foolish as the behaviour of using language which means that neither he nor anyone else has a mind.

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NATURE'S ARCHITECTURE.

By Professor Sir W. H. Bragg, K.B.E., D.Sc., F.R.S.

We have always the wish to examine closely the substances which we handle in everyday life, and to enquire into their natures and their properties. When we design and construct we find it necessary as well as interesting to consider with care the materials with which we build. As our knowledge advances our study is carried further and further into the minute: and it becomes ever more important. Thus, for example, the metallurgist uses the microscope to examine the arrangements of the minute crystals in his metals and minerals; the biologist seeks to discover the cellular structure of living organisms, or to watch the movements of bacteria which are far beyond unaided vision. There is a world of the microscope in which men follow processes that underlie the larger effects that we see, and, so doing, widely extend our understanding and our powers. We have only to consider for a moment the gaps that would be made in our knowledge, were we to take away the microscope and all that it has revealed to us, in order to realize the importance of the study of the very small. We should lose by far the greater part of our inner knowledge of biology and pathology, of botany, of mineralogy, of other sciences: and should miss the innumerable applications of the microscope in arts and industries.

All our experience in the study of the minute, and every indication that we have, directs us to the usefulness of pushing on still further. With every step forward we see more of the wonders of the world and grow in our sense of its mystery and its immensity: and we come back with our hands full of things that we may admire, and, if well, put to our service.

There is a limit to the power of the microscope; a limit which is unsurmountable. The waves of light by which we see are of the order of a ten thousandth of a centimetre: objects that are to be seen must at least be of similar dimensions. A small rock makes no permanent impression, has no more than a local effect on an ocean swell that sweeps over it. If we are to press on beyond the range of the microscope we must abandon the use of ordinary light.

Now we have of late acquired a new instrument with which we can measure distances and spacings ten thousand times smaller than have ever been measured before. It uses X-rays where the microscope uses light. It carries us deep down into the fundamental structure of Nature; where are the very elements of which physics and chemistry have told us. We see Nature as an architect, fitting together these elements into the structures that, when they have grown sufficiently, are the materials that we know. I would like to-night to show you something of this world, though our vision is still new and indistinct.

I do not propose to describe in detail the way in which X-rays are employed in this new field. It would take too long to do so, and I am anxious to describe to you results rather than the methods of getting them. It is sufficient to say that just as light and its waves are used in the investigation of structures whose dimensions are of the same order of magnitude as the waves, so, with X-rays we are able to examine and measure lengths which are proportionally smaller.

We are not able to apply X-rays to the study of a single atom, nor of a group of atoms, because their effect is too small. But we are fortunate enough to find in the crystal an ordered arrangement consisting of innumerable repetitions of some fundamental group. The unit may be no more than one or two atoms of the same or different kinds arranged in a particular pattern. When this unit is infinitely repeated, each in its proper space so that one unit of pattern is exactly like every other, then, the crystal forms a solid whose faces and angles are founded upon and display to our eyes some of the elementary features of the pattern. It is then, too, that the X-rays have enough material to work upon and can tell us the fundamental structure and measure its dimensions. That is why the crystal is of such immense importance. If bodies consisted of atoms jumbled together just anyhow, then the new X-ray methods would be unable to help us; we depend on the regularity and the sufficient repetition of the crystal. We are able to infer the fundamental details from the behaviour of the whole.

The X-ray methods of analysis tell us with great exactness the dimensions of the little cell in which the unit of pattern is contained. Just as we can divide up a pattern on a paper or a piece of material by two sets of parallel straight lines, so that our little four sided figure contains one unit of pattern and no more, so we can divide up space into elements of volume with six faces which are parallel in pairs and which contain one set of atoms that make up the fundamental pattern. By means of X-rays we can measure these cells; we can find their dimensions and their angles; we can even find the places of the atoms within the cells and so discover the way in which Nature has put the atoms together; we can find the plan on which the crystal is built. Not that as yet we possess the skill and knowledge to determine the structure of every crystal but in every case we can do a little and in some of the easier cases we can find the details with great accuracy. We know now how the atoms are put together in many of the simpler compounds such as rocksalt and all the other substances which are like it; of diamond, of zinc blende which resembles the diamond, of carborundum and others of the same family; of ice; of the carbonates; of the metals and so forth. We have learnt some of the rules of building and something of the nature of the atoms as told us by the part that they play in the structure. The day may not be so far away when we can anticipate a natural design and say that if we took such and such atoms and put them together they would unite in this or that fashion, and we might describe the physical properties of the crystal as natural consequences of the structure. Even now we can try our powers. Let me state some of the building rules and let us see how some of the simpler constructions are derived from them.

First let us understand the dimensions of the world in which we are working. Let us imagine that we have altered the scale of our surroundings in the proportion of one hundred millions to one. Then a tennis ball has become as large as the earth; and the room in which we stand has grown so as to take in the moon and far beyond. We can see the atoms now: some of the size of marbles, some as big, perhaps, as golf balls. In shape we conceive them to be round, more or less: though we have indications that some are of spheroidal, or even of less symmetrical form. It may be that when we see better we shall be able to describe them more precisely. For the present, we are obliged to choose some form of representation in our models and for convenience we make them spheres. They are laced and bonded together into structures of lace-like pattern, and we can apply a measuring rod to some at least of the distances between centre and centre.

As Chemists have already told us there are nearly one hundred elements of different kinds varying in weight from the lightest—hydrogen—to the heavier atoms of mercury, lead, uranium and others. We begin to be aware from the knowledge that the study of X-rays has given us that they are not merely graded in size and weight but that in construction they are all singularly alike. Each possesses a central core in which the massiveness of the atom seems to lie. The core is positively charged, to speak in electrical terms; and from atom to atom the electrical charge amounts by uniform steps. If we say that hydrogen has a charge of 1 in its nucleus, then helium has a charge of 2, lithium of 3; next come beryllium and boron, then come carbon with 6; nitrogen 7, oxygen 8, fluorine 9 and so on. Round about its core each atom has a group of bodies of negative charge which we call electrons. The charge on each is equal in magnitude though opposite in sign to that of the positive charge on hydrogen and the number belonging to each atom is the same as the number denoting the positive charge on the core. Each atom, therefore, has no excessive charge of either sign, as a whole it is neutral, and the electrical charges which it possesses compensate each others effect except locally at points so near the atom that the balance is not complete. Thus, we become aware that all the elements of which all things in the world are made are of one and only one pattern differing from each other only in a certain fundamental number. All the chemical properties of an atom are, it seems, fixed by this number. Coal is coal and burns in the grate because the carbon atom is number 6; that is to say, it has a positive charge 6 on its core and has 6 electrons. Oxygen is somehow a gas of immense importance to the living body because its number is 8.

When we look a little closer into the atoms and their electrons we observe that there is order in the arrangements of the electrons round the core. The electrons of helium are disposed at opposite poles. We do not know for certain to what extent they move. Some things we can explain best by supposing them to be in motion and other things suggest to us that they are in rest. Here is a point that we do not as yet see clearly but we know for certain that there are two electrons, and that on the whole they are oppositely placed. When the next electron is added and the core is correspondingly strengthened, the new substance, which is the metal lithium, begins a new arrangement of the electrons.

From this time onwards the electrons after the first two arrange themselves on the surface of a spherical shell which contains the core and the first two. Thus, for example, carbon has two inside and four on its shell. So the increase goes on until the number on the shell is eight, there being still two inside and the charge on the core being now equal to ten. This substance is neon, one of the rare gases discovered by Sir William Ramsay in pursuance of the investigation which he made after Lord Rayleigh and he had discovered the existence of argon. Continuing, a further addition of electrons is made to form another shell which surrounds the first. When eight more have been added we come to gas much resembling neon, the already mentioned argon. Two shells are now complete; and yet another electron shell begins as other atoms are added to the list and so on. For the moment we have done enough; the attached table contains a summary of our descriptions.

TABLE I.

		Charge of the Core	Electrons in			
			Shell I	Shell II	Shell III	Shell IV
1.	Hydrogen	1	1	—	—	—
2.	Helium*	2	2	—	—	—
3.	Lithium	3	2	1	—	—
4.	Beryllium	4	2	2	—	—
5.	Boron	5	2	3	—	—
6.	Carbon	6	2	4	—	—
7.	Nitrogen	7	2	5	—	—
8.	Oyygen	8	2	6	—	—
9.	Fluorine	9	2	7	—	—
10.	Neon*	10	2	8	—	—
11.	Sodium	11	2	8	1	—
12.	Magnesium	12	2	8	2	—
13.	Aluminium	13	2	8	3	—
14.	Silicon	14	2	8	4	—
15.	Phosphorus	15	2	8	5	—
16.	Sulphur	16	2	8	6	—
17.	Chlorine	17	2	8	7	—
18.	Argon*	18	2	8	8	—
19.	Potassium	19	2	8	8	1
20.	Calcium	20	2	8	8	2

* The inert gases.

We have described our atoms. What are we to say as to the methods by which they are to be attached one to another?

An atom which requires one or two or more electrons to complete a shell manifests a great determination to seize them and, so to speak, round off its surface structure. On the other hand, an atom which is an early member of a new series, that is to say, in which one shell is complete and one or two or three of the next shell have been added, has but a light hold on these extra electrons and gives them up when the demand is sufficiently pressing. Let us add to this that if an atom is given an electron over and above what it ought to have it is now negatively charged as a whole, if it has lost one it is positively charged. These are our main rules: there are others no doubt but we have sufficient to go on with and we can start our constructions.

Let us begin with a very simple case—rocksalt The crystal is made of equal numbers of sodium and chlorine atoms If we examine the attached table we see that chlorine is the substance just short of argon requiring yet one more electron to complete shell III The metal sodium is a substance which has two complete shells and one extra electron on which its hold is weak Let a chlorine and sodium come within acting distance of one another and the obvious happens The chlorine seizes the loose electron of the sodium The chlorine is now negatively charged the sodium positively and the two atoms tend to keep together by virtue of this charge Moreover if there are a number of atoms of each sort each positive will surround itself with as many negatives as it can and each negative with the same number of positives under these circumstances the structure adopted is that shown by the model (fig 1) There is a cubic arrangement in which every line of atoms parallel to the edge of the cube consists alternately of sodium

Fig 1

and chlorine, and in the crystal as a whole each atom has six neighbours of opposite sign. Uncombined the chlorine was aggressive and, chemically, very active, it has now settled down. So also the sodium uncombined had special properties due to its possession of a lightly held electron. The sodium metal is, for example, a good conductor of electricity like other metals, because the electrons can easily be driven through the body of the metal under the action of electromotive force and in this way a transfer of electricity can be effected or in other words, a current of electricity can readily be set in motion. At any time the sodium atom is liable to be attacked by one of the atoms which still require one or more electrons to complete a shell. For example, sodium put into water is at once attacked and a compound is formed in which sodium is allied with oxygen and hydrogen.

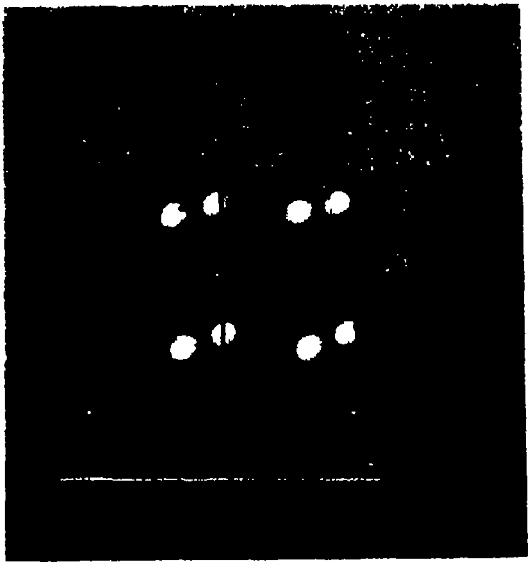


FIG 2.

There are many instances of this kind of structure. Sodium may combine with bromine or fluorine just as well as with chlorine. Potassium behaves like sodium. In every case the result is the same, namely, the building of a cubic structure in which each atom of one kind is surrounded symmetrically by six atoms of the other.

Let us see what happens if an atom which has two electrons to spare meets others which each need one to complete a shell. A beautiful instance is given by the common mineral fluorspar. Here a calcium atom has given up each of the two electrons which are lightly attached to it, and two fluorine atoms each take one. In the structure of the fluorspar crystal the arrangement is to be such that the calcium is to be surrounded by twice as many fluorine neighbours as the fluorine by neighbours of calcium. It is very interesting to see how Nature has solved the problem. A pattern of the crystal is shown in figure 2. Every calcium has eight fluorine neighbours; every fluorine four calcium neighbours. The crystal is highly symmetrical and is indeed placed by crystallographers in their highest class. It is to be remembered that in such a model as is illustrated by the figure, it is impracticable to make it include more than a few atoms of each kind. One has to imagine it extended in space in all directions after the pattern already begun in order to appreciate the exact relations of the atoms to one another. The structures of ice, of ruby, of cuprite and many other substances show the same effort to adjust the number of neighbours to suit the proportions in which the atoms are put together.

We now take an entirely different class of structure. It is common knowledge that many solid substances and crystals are compounded of atoms which cannot be expected to transfer electrons in the way described. Carbon atoms, for example, compound to make a diamond.

It is proposed by some that combinations of atoms of this kind, which obviously occur, are to be thought of as a sharing of electrons. The atoms lie up against one another in such a way that one or more electrons do duty in completing shells of two neighbouring atoms, just as a party wall is common to two semi-detached houses. To say the least the idea is very helpful and enables one to form a mental picture of what is occurring. Two oxygen atoms, for example, may be thought of as sharing electrons in this way so as to make one molecule. The single oxygen atom is greedy for electrons. The combined molecule consists of two atoms which have four electrons in common and have in an economical way satisfied their desires. The molecule wants little more; it is very independent, has no desire to associate with members of its own kind at ordinary temperatures and so is a gas; that is to say, a substance in which the molecules prefer an independent existence. The same is true of nitrogen which forms the principal constituent of the atmosphere. Argon which the air also contains is an atom with a shell which is naturally complete. It is impossible to get it to enter into combination with any other kind of atom since it has neither electrons to give away nor does it want any addition to what it possesses: that is why it is a gas and in particular why it remained so long unknown. It took no part so to speak in the affairs of the universe and it was overlooked.

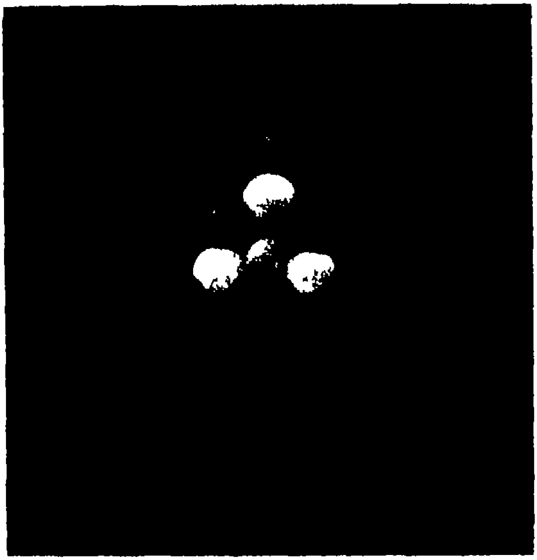
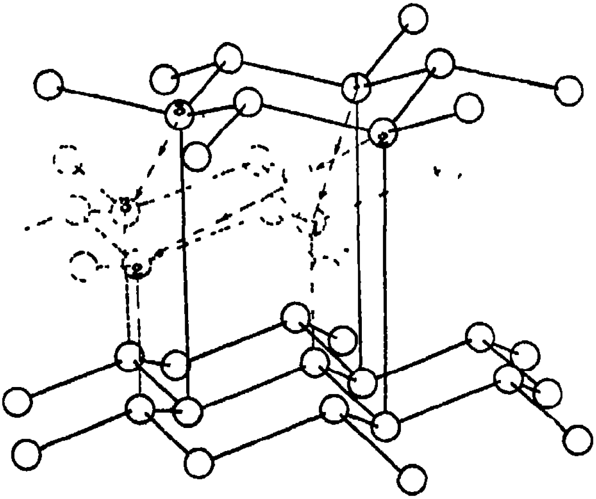


FIG. 3.

The way in which the carbon atoms share their electrons is most interesting. Each carbon atom borrows and lends one to each of four neighbours; in this way its own four are artificially raised to eight, which is the perfect number. The structure of diamond is such, therefore, that each atom is surrounded by four neighbours at equal distances (see fig. 3). This one distance is the one and only dimension in the diamond; its magnitude is 1 54 A.U. The whole of the diamond is bound together in this way, one cannot say where the molecule begins or ends. The whole crystal, in fact, is one molecule. Electron sharing makes a very strong bond between the partners. It is very difficult to separate atoms so tied together and the diamond, in which this bonding is universal is the hardest of known substances.



The Firm Lines of the Diagram show the Structure of Graphite By Moving the Top Layer to the Position shown by the Broken Lines the Diamond Structure is obtained

Fig. 4.

It is very instructive to compare diamond with graphite because the two crystals differ so remarkably in all their properties and yet each is composed simply of the one element carbon. Diamond is transparent and is the hardest of known substances; it will scratch anything. Graphite is opaque to light and is used as a lubricant. What remarkable difference in construction accounts for the great contrast in properties?

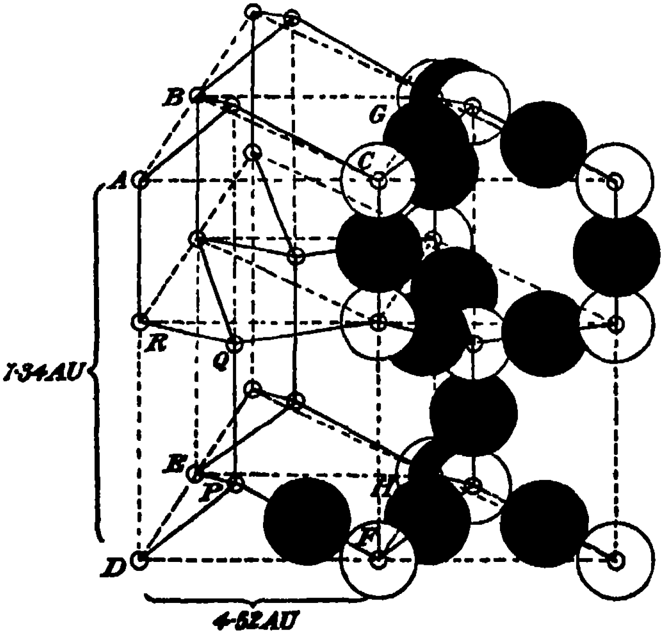
The methods of X-ray analysis show that there is an alteration which is illustrated by the accompanying figure (fig. 4). Without models or other illustrations it is difficult to realize how we must move the atoms in diamond so that the structure becomes that of graphite and a verbal description is not easy to follow. Perhaps the best way to describe the change is as follows:—

Let successive sheets of the carbon atoms in diamond, which are parallel to a natural face, be moved away from each other without altering the relative positions of the atoms in the sheet. In consequence the bonds between the atoms in any one sheet are as strong as ever, while each sheet is now lightly attached to the next because the distance between two sheets is nearly doubled. Separate sheets can now slide over each other very easily: hence the lubricating power of graphite. It is, however, to be observed that it is not only the weakness of the ties between sheet and sheet which are the cause of the slipperiness but also the tightness of the bonds in each sheet which hold the sheet together. The substance is flaky, and the flakes slide on one another easily and divide readily into smaller flakes, but the flakiness is a very persistent property. If the flakes themselves broke up into individual atoms the substance would be a mere powder and would not lubricate at all. Let us notice that the characteristics of graphite, its slipperiness and flakiness are the expression on the large scale of the primary details of construction because this is a point on which I should wish to insist. Its opacity is more difficult to explain completely but at least we can say this that in diamond all the atoms are rigidly bound together, it rings true like a bell to the minute waves of light, while the graphite is full of innumerable loose ends and, just as a bell full of cracks absorbs the energy of a blow and turns it into heat without giving out sound waves into the air so the graphite absorbs the energy of the light without transmitting it.

We took up the construction of the diamond just now because it illustrates particularly well the phenomenon which we call electron sharing. There are other very striking instances of the same effect to be found in many of the simpler crystals. Iceland spar or calcium carbonate is a very common mineral. The element of the pattern includes one atom of calcium, one of carbon and three of oxygen. Calcium is number 20 in our atomic scale: it has completed two shells containing 8 electrons each and has 2 more towards the next shell. It is a metal, therefore, which has electrons to spare and conducts electricity. The carbon and the three oxygens are each and all short of electrons. The difficiencies are far greater than the calciums can supply, considering, at any rate, the proportion in which the atoms are mixed; but the carbon and the oxygens, with much sharing of electrons between themselves try to satisfy their desires and very nearly succeed. They make a disc-like arrangement with the carbon in the centre and the three oxygens in a plane round about the carbon. Having done their best they are still 2 electrons short and that is where the calcium fills a want. It hands over to the CO_3 group the 2 electrons it can spare and so the arrangement is like that of the ordinary salt except that the transfer of electrons is in twos and not in units and that the group of carbon and three oxygens is not spherical as the single atom of chlorine was in salt. Again, however, the metal, now positive in charge, surrounds itself with as many negatively charged neighbours as it can and vice versa. The structures of calcium carbonate and many other carbonates, which have the same general basis, are exactly the same as that of salt but they have lost the cubic form because the CO_3 group is not round.

There are many crystals built up on this pattern. They consist of a metal which gives up one or two electrons and a negative counterpart which may be a single atom or two atoms or three or more combining amongst themselves to form groups which lack only the electrons that the metal is able to supply. Each group is tightly bound together; the bonds between the metal and the group may not be so strong, but are still very much stronger than the bonds often found to be holding a crystal together; as for example, in the case of the organic substances. Consequently the latter are softer in comparison, still more so in comparison with crystals formed throughout on the electron sharing plan. To this first-named class of substance belong sulphates like copper sulphate, metallic oxides and so forth, and generally speaking the class known as the polar compounds. It is an essential feature of these substances that the molecule cannot be distinguished in the crystal. A positive has several negative neighbours and has no preference for one rather than for another. We cannot link together a certain positive and a certain negative and say, " this is the molecule."

The same sort of structure is found in ice. Here an oxygen atom wanting two electron compounds with two hydrogen atoms each of which, you will remember, has but one. With the rules we have laid down we can be very nearly sure of the structure that ice must have before subjecting it to any X-ray analysis at all. We anticipate that we may find a structure in which each oxygen is symmetrically surrounded by hydrogen neighbours and each hydrogen by oxygen neighbours but the oxygen is to have twice as many neighbours as the hydrogen. We had such a case in fluorspar, where the calcium had eight fluorine neighbours; that, however, cannot be the structure of ice because it gives too dense a crystal, unless we space the atoms very far apart, more than their known dimensions will allow. Ice is a very light substance. The atoms of which ice is made must be sparsely distributed in space. The distribution must be one in which the number of neighbours is reduced to a minimum and that is the distribution in which each oxygen has 4 hydrogen neighbours and each hydrogen 2 neighbours of oxygen. One cannot imagine an atom surrounded by less than 2 others. Now the diamond structure is one in which every atom has 4 neighbours. Following out this idea we place an oxygen atom where we should place a carbon atom in the diamond but we put a hydrogen in between each pair of oxygens. We have now one slight change to make which does not alter any of the distances but changes the cubic crystal into a hexagonal; for we know that the ice crystal is a hexagonal column. The result is the form which is illustrated in fig. 5: it is very beautiful in design and very simple. The extraordinary emptiness of the structure is perhaps its most striking feature.



BLACK CIRCLES REPRESENT HYDROGEN ATOMS; WHITE CIRCLES REPRESENT OXYGEN ATOMS.

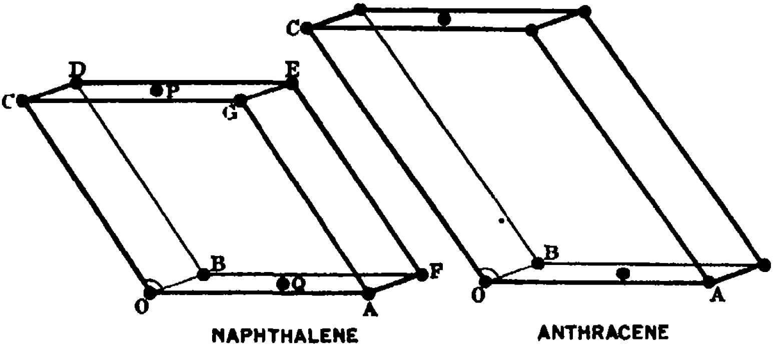
Fig 5.

If we now compare the figure to which we have been led by trying to use the rules laid down with the actual ice crystal as examined by X-ray methods, we find an excellent agreement. In fact, independent methods give practically the same result. Here again let me draw your attention to the fact that the dispositions of the atoms in the elementary cell forecast the properties of the whole substance. The feathery snow crystal with its six-pointed star can already be seen in the model which we have made.

Lastly, let me describe to you some of the things we have recently learnt concerning the crystals of the organic substances: those which are so essential to living organisms. They are complicated in structure and yet the organic chemist has been able to picture to himself certain fundamental laws as to the relative arrangements of the atoms which have opened out a field not only of great interest and beauty but also of immense importance in practice. The X-rays now come, we hope, to the aid of the organic chemist, giving him the power of applying a measuring rod to the structures he has imagined and thereby acquiring a vastly superior insight into their composition and properties.

There are two classes of these organic substances. One is distinguished particularly by the formation of long chains of atoms, particularly of carbon atoms. To this class belong oils and fats, alcohols, ethers, sugars and so forth. In the second great group the essential feature is the combination of six carbon atoms into a ring, the famous benzene ring. The complex molecules contain one or more of these rings as the basis of their structure. To this class belong the aromatic substances, the sweet smelling essences which give the class its name, as well as toluenes, naphthalenes, anthracenes, quinines, many of the dyes, and so forth.

Analysis by X-rays shows that these ring arrangements actually exist and have definite dimensions which they retain unaltered from substance to substance. If it were not so we should indeed have little chance of solving the problem of organic structure. In naphthalene, for example, the unit of pattern is made up of ten carbons and eight hydrogens. It would be a hopeless task with our present means and our present knowledge to solve the problem of the arrangements of 18 atoms in the pattern, but we refer back to the diamond and graphite structures, and argue that in both cases we see rings of six carbon atoms bound together. We assume that the framework of six atoms which we see repeated again and again in the structure of graphite can be maintained if by chemical means the graphite is broken up. We measure the sizes of these rings in graphite and we try to fit them into the cells whose dimensions we determine by X-ray methods. In fig. 6 is drawn to scale the naphthalene cell which we find has to contain two molecules of naphthalene. The naphthalene molecule consists of two rings as in the figure and hydrogens are attached to all the carbons but two. We try whether we can conveniently fit together blocks of this size into the cell provided, and are encouraged to find that it can be done very well. Moreover, by comparing one substance with another we can find hints as to the distribution of the blocks within the cells. For instance, an anthracene cell is almost exactly the same in all respects as the naphthalene cell except that one of its edges has grown considerably in length. Now examine the structure of these two. We find that one main difference is the fact that there are three rings in a row in anthracene and only two in naphthalene. It is most probable, therefore, that the length of the molecule in each case lies along the side of the cell which shows a difference in the two cases. It is an additional confirmation that the increased length in anthracene is approximately the width of a benzene ring as we deduce it from measurements of graphite or diamond.



Unit Cells of Naphthalene and Anthracene Drawn to the Same Scale

	$OA = a$	$OB = b$	$OC = c$
Naphthalene . ..	8·34	6·05	8·69
Anthracene	8·7	6·1	11·6

Naphthalene $\alpha = BOC = 90°$, $\beta = COA = 122° \, 49'$, $\gamma = AOB = 90°$

Anthracene $\alpha = BOC = 90°$, $\beta = COA = 124° \, 24'$, $\gamma = AOB = 90°$

Fig. 6.

This part of our work has not gone far as yet, it is a new field of research. We have not even entered into the measurements of the first class of organic substances. We have measured only a few of the crystals whose molecules contain one or more benzene rings. As the different forms of organic crystals can be numbered in many thousands, there is yet plenty to do.

A very striking feature of the crystal of the organic substance is that in this case it is possible to see the whole molecule within the crystal: the bonds between the molecules are strong but the attachment of any molecule to the next is weak. As I have already stated the positive and the negative portions of an atom cannot counterbalance each others effects at every point close to the atom: there are what the electrician calls "stray fields of force." They are far weaker than this force which binds together two atoms which are sharing electrons and even than those electrical forces which bind together the positive and negative in a crystal like rocksalt. Round about the fringe of a naphthalene molecule these stray fields exist at various points. When a molecule of naphthalene is laid up in the correct way against another molecule of the same substance, these stray fields interlock and the molecule settles down to a permanent attachment. As molecule is added to molecule, the crystal grows but the forces in this kind of combination are weak and that is why the substances are so soft and can so easily be melted.

Another very striking point is that two molecules attach themselves together side by side with a little more force than end to end. When a naphthalene crystal grows the molecules tend first to lock together side by side and so the substance grows in thin sheets and flakes. Naphthalene is a substance which can crystalize from its vapour, and when it does so it tends to form a very light open structure because the sheets spread sideways and join themselves together in various ways. The cleavage of the naphthalene crystal is across the ends of the molecules because these are the weakest of all the forces which bind the molecules together and are first to give way. This side to side attachment seems to be one of the main reasons why a film of oil spreads so quickly over the water. Langmuir and others have shown that the thinnest films of oil which form a complete covering on the water's surface consists of a side to side arrangement of the chain molecules of the oil. The lock molecules are like the fibres of a pile carpet.

In a substance such as oleic or palmitic acid, there is at the end of each molecule a group of atoms which has a strong attachment for water and so to speak takes root in the water's surface. It is to be presumed that when a drop of oil is placed on the water's surface some of the molecules attach themselves at once at their ends and are held while the other molecules, exerting their side to side forces quickly fit themselves into place. They also take root in the water and join up sideways with the molecules already in position, so the film spreads very quickly.

I have tried by these few instances to explain the trend of some recent lines of research. It opens up, I think, a most fascinating field of enquiry. Below the infinite complexity of material substances, of the crystals which reveal most directly the fundamental molecular structure, and still more of all the variety of other substances which consist also of crystals but so mixed and broken as to be indistinguishable by the naked eye: below all this world of infinite variety lies another in which are simplicity and regularity and exquisite perfection of form. We have acquired new powers for insight into that which goes on in these depths. We can so to speak wander about among the various atoms whose variety is not so great as to frighten us and we can pull and push them about into different places, fitting them together into this and that structure trying over the architectural designs on which the material world is based.

THE MECHANISM OF α, β AND γ-RAY EMISSION

By E. Madgwick, M.C., M.Sc.

(Read March 2nd 1922)

It is proposed to examine in relation to one another the properties of the α, β and γ-rays from the radioactive substances, with the object of investigating the mechanism underlying their emission.

That there is some connection between the β and γ-rays seems certain from the fact that they are generally emitted together. Their resemblance to the cathode and X-rays respectively suggests that the γ-rays are produced by the stoppage of β particles, just as the X-rays are generated by the stoppage of cathode particles; the only difference being that whereas the cathode particles are shot into the radiating substance, the β particles have their origin within the substance itself. Experiment shows that the energy E of a cathode particle can be transferred to an X-ray of frequency ν in accordance with Planck's quantum relation.

$$E = h\nu \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where h Planck's constant $= 6{\cdot}55 \times 10^{-27}$ ergs, and

$$E = m_0 c^2 \left(\frac{1}{\sqrt{1-\beta^2}} - 1 \right) \quad . \quad (2)$$

m_0 = mass of electron at slow speeds.

c = velocity of light.

v = velocity of electron.

$\beta = \frac{v}{c}$

The velocities of the different groups of β-rays from Radium B and Radium C have been measured by Danysz (C.R., 1911) and by Rutherford and Robinson (*Phil. Mag.*, 1913). The wavelengths of the γ-rays from the same substances have been determined by Rutherford and Andrade (*Phil Mag* 1914) who used as a source a tube of Radium Emanation When the energies of the β particles and of the γ pulses are calculated from equations (2) and (1) it is found that the spectra do not overlap to any very considerable extent This is to be expected owing to the difficulty of measuring the wave lengths of hard γ-rays Taking only the velocities representing energies within the range common to both spectra the following are the corresponding wave-lengths λ as found from (1)

TABLE I

RADIUM B		RADIUM C	
β	$\lambda \times 10^9$cm	β	$\lambda \times 10^9$cm
656	758	67[illegible]	692
635	825	648	787
489—475*	1 67—1 80	632	840
453—443	2 01 2 11		
426	2 32		
414	2 49		
365	3 33		

* Groups

Whiddington has shown (*Phil Mag* 1920) that the minimum velocity of the cathode rays necessary to excite the characteristic X-radiation is

$$\left.\begin{array}{llll} \text{for the K series} & v = 2\,(N-2)\times 10^8 & \text{cms/sec} \\ \quad ,, \quad \text{L} \quad ,, & v = (N-15)\times 10^8 & ,, \end{array}\right\} \quad (3)$$

N being the atomic number of the radiator Combining these with (2) and (1) the characteristic K and L wave-lengths are obtained In the following table these are shown for Radium Emanation and its products Although it is known that the emanation and Ra A do not of themselves emit γ-rays, it is a necessary consequence of our present hypothesis that their characteristic radiations would be excited by the β particles from the Ra B and Ra C.

TABLE II.

Radiator.	Atomic Number N.	$\lambda_K \times 10^{9}$cm.	$\lambda_L \times 10^{9}$cm
Ra Em.	86	1·18	8·17
Ra A	84	1·26	8·76
Ra B	82	1·35	9·44
Ra C	83	1·31	9·09

The extent of the concordance between these results and the wave-lengths as determined by Rutherford and Andrade is shown below.

TABLE III PENETRATING RAYS.

Ruth and Andrade's results	λ from Table I	Characteristic K radiation.
·72 × 10^{-9}cm.	·692 × 10^{-9}cm.	
·99 ,,		
1·16 ,,		1·18 × 10^{-9}cm. (Ra Em.)
1·37 ,,		1·35 ,, (Ra B.)
1·59 ,,		
1·69 ,,	1·67 ,,	
1·96 ,,	2·01 ,,	
2·29 ,,	2·32 ,,	
2·42 ,,	2·49 ,,	
2·62 ,,		
2·96 ,,		
3·24 ,,	3·33 ,, .	

TABLE IV SOFT RAYS

Ruth and Andrade's results	Characteristic γ radiation
793 × 10^{-8}cm	
809 „	817 × 10^{-8}cm (Ra Em)
838 „	
853 „	876 „ (Ra A)
917 „	909 „ (Ra C)
953	944 „ (Ra B)

It is perhaps worthy of note that the wave length 845 × 10^{-8} cm corresponds to an element of atomic number 85

Taking Danysz's results Rutherford has shown ("Radioactive Substances and their Radiations," p 613) that for a number of groups the energy differences can be represented by the relation pE_1+qE_2 where $E_1 = 0\ 456 \times 10^{13}e$, $E_2 = 1\ 556 \times 10^{13}e$, and p and q are whole numbers The figures are contained in the following table, those in the last column being calculated by substituting for E_1 and E_2 in the expressions of the preceding column

TABLE V

Number of group	Difference in energy	$pE_1 + qE_2$	Calculated
(21) — (20)	45 × $10^{13}e$	E_1	456 × $10^{13}e$
„ — (19)	1 37 „	$3E_1$	1 37 „
„ — (18)	1 56 „	E_2	1 56 „
„ — (17)	1 84 „	$4E_1$	1 82 „
— (16)	2 05 „	E_1+E_2	2 01 „
„ — (15)	3 11 „	$2E_2$	3 11 „
„ (14)	4 03 „	$2E_1+2E_2$	4 02 „
„ — (13)	4 48 „	$3E_1+2E_2$	4 48 „
„ — (12)	4 92 „	$4E_1+2E_2$	4 94 „
„ - (11)	6 03 „	$3E_1+3E_2$	6 03 „

If the energy associated with the γ pulse is derived from the β particle in accordance with equation (1), then it might be anticipated that a similar relationship would hold for a number of lines in the γ-ray spectrum This is found to be the case for the eight shortest wave-lengths of Rutherford and Andrade

TABLE VI.

No	λ	Energy $= \frac{h\nu}{e}$	Difference in energy	$pE_1' + qE_2'$	Calculated
1	72×10^{-9} cm	$1\ 73 \times 10^{13}$			
2	99 ,,	1 26 ,,	(1) (2) 47×10^{13}	$2E_1' + E_2'$	46×10^{13}
3	1 16 ,,	1 08 ,,	,, (3) 65 ,,	$E_1' + 4E_2'$	65 ,,
4	1 37 ,,	91 ,,	,, (4) 82 ,,	$2E_1' + 4E_2'$	82 ,,
5	1 59 ,	79 ,	,,-(5) 94 ,,	$2E_1' + 5E_2'$	94 ,,
6	1 69 ,	74 ,,	,, (6) 99 ,,	$3E_1' + 4E_2'$	99 ,,
7	1 96 ,,	64 ,,	,, (7) 1 09 ,,	$5E_1' + 2E_2'$	1 09 ,,
8	2 42 ,,	52 ,,	,, (8) 1 21 ,,	$5E_1' + 3E_2'$	1 21 ,,

$E_1' = 17 \times 10^{13}e$ $E_2' = 12 \times 10^{14}e.$

To explain this relationship it may be assumed that the β particle is emitted with constant velocity, and that in order to pass through each ring of electrons it must give up a definite amount of energy characteristic of the ring, the total energy lost during successive operations being $pE_1+qE_2+rE_3$. But if this energy is radiated in the form of γ-pulses there ought to be present in the γ-ray spectrum strong lines of wave-lengths

$2\ 745 \times 10^{-9}$ cm corresponding to $E_1 = 456 \times 10^{13}e$
and 804 ,, ,, $E_2 = 1\ 556$,,

These are not represented

Moreover it is found that the differences between the energies of the α particles emitted by the Uranium-Polonium series can similarly be represented by an expression pE_1+qE_2

TABLE VIII

No	Product	Kinetic energy (ergs)	Energy differences		$pE_1'' + qE_2''$	Calculated
1	Uranium 1	645×10^{-5}			–	
2	,, 2	72 ,,	(2)–(1)	075×10^{-5}	$E_1' + E_2$	074×10^{-5}
3	Ionium	746 ,,	(3) ,,	101 ,,	$2E_1' + E_2^1$	101 ,,
4	Radium	794 ,,	(4) ,,	149 ,,	$2E_1'' + 2E_2''$	148 ,,
5	Emanation	915 ,,	(5) ,,	270 ,,	$3E_1^1 + 4E_2$	270 ,,
6	Ra A	1 01 ,,	(6) ,,	365 ,,	$3E_1^1 + 6E_2$	366
7	,, C	1 31 ,,	(7) ,,	665	$9E_1' + 9E_2^1$	666
8	,, F	866 ,,	(8) ,,	221 ,,	$3E_1 + 3E_2$	222 ,,

$E_1' = 026 \times 10^{-5}$ $E_2' = 048 \times 10^{-5}$

A similar explanation might be applied to this case on the assumption that the α particle originates within the nucleus with definite velocity and gives up an amount of energy characteristic of the nucleus in its passage out But as will be shown below, the energy of the α particle can be accounted for on the assumption that it is shed from the *outside* of the nucleus so that, in Table VIII at least, the $pE + qE$ relationship appears to be either accidental or due to some unknown cause

On Rutherford's nuclear theory of the atom, derived from considerations of the scattering of α particles on their passage through matter, the atom consists of a positive nucleus of charge Ne and radius of the order 10^{-12} cm surrounded by N electrons within a sphere of about 10^{-8} cm radius Suppose first of all that the α particle starts from rest at a distance r from the centre of the nucleus and that its velocity is acquired simply as the result of the mutual repulsion in accordance with the law of inverse square between the charge Ne of the nucleus and the charge $2e$ of the α particle The energy of expulsion is then

$$E = \int_r^\infty \frac{Ne \times 2e}{r^2}\, dr = \frac{2Ne^2}{r} \tag{4}$$

also $E = \frac{1}{2} MV^2$ (5)

where M = mass of α particle = $6\,6 \times 10^{-24}$ gm

V = velocity.

Values of the velocities of the α particles have recently been published by Geiger[*] (*Zeitschrift für Physik*, December, 1921). From his results the value of r corresponding to each value of V has been calculated from (4) and (5). In the following table is also shown the number of electrons in the nucleus, obtained by subtracting the atomic number (the net number of positive charges) from the atomic weight (the actual number of protons).

TABLE IX.

Product	$V \div 10^9$ cm	$E \times 10^6$ (ergs)	N	$r = \frac{2Ne^2}{E}$	Atomic weight	Electrons in nucleus
Uranium 1 .	1·396	6·334	92	6·416 × 10^{-13} cm.	238·5	146
,, 2 ..	1·462	6·949	92	5·848 ,,	*234·5	142
Ionium	1·482	7·141	90	5·567 ,,	230·5	140
Radium .	1·511	7·422	88	5·238 ,,	226	138
Ra. Em.	1·618	8·467	86	4·486 ,,	222	136
,, A	1·688	9·262	84	4·006 ,,	218	134
,, C .	1·922	12·00	83	3·056 ,,	214	131
,, F ..	1·587	8·191	84	4·530 ,,	210	126
Thorium	1·435	6·691	90	5·942 ,,	232	142
Radioth	1·600	8·320	90	4·778 ,,	228	138
Th. X	1·643	8·772	88	4·432 ,,	224	136
,, Em.	1·728	9·703	86	3·915 ,,	220	134
,, A	1·796	10·49	84	3·538 ,,	216	132
,, C	1·696	9·353	83	3·921 ,,	212	129
,, C'	2·063	13·83	84	2·683 ,,	212	128

* Isotopes of atomic weights 238, 234 and 230 respectively are assumed.

The α particle is constituted of four protons and two electrons. If the electron be regarded as a charged sphere, and if, as the experimental evidence indicates, its mass is electromagnetic,

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[*] Those in Table VIII. were taken from Rutherford's "Radioactive Substances and their Radiations." The earlier part of this paper was written before the publication of Geiger's results.

$$a = \frac{2}{3}\,\frac{e^2}{m_e} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where a is the radius of the electron. It will be seen from (6) that if these two properties be attributed also to the proton, the radius of the proton is 1800 times less than that of the electron, since its mass is 1800 times greater. On this view it would

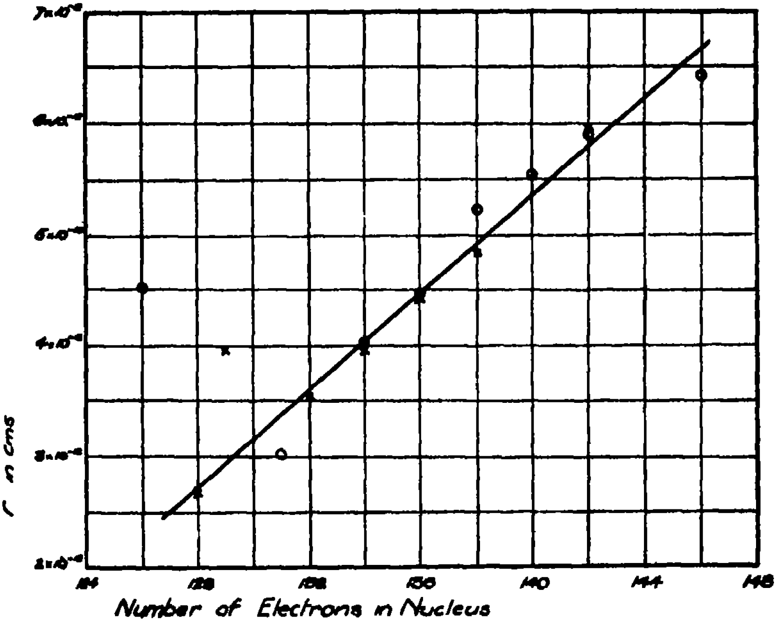


FIG. 1.

appear that the size of the α particle (regarded merely as the space it occupies and having no reference to the force it exerts) is practically identical with that of the two electrons it contains. Thus if there is any ordered arrangement of the α particles in the nucleus before emission the r in (4) should be a function of the number of electrons in the nucleus. The two quantities are plotted against one another in Fig. 1, the Uranium series being shown by circles, the Thorium series by crosses. Neglecting for the moment the points corresponding to Ra. F and Th. C, these lie roughly about a straight line the slope of which indicates that for each electron emitted by the nucleus r decreases by about $2{\cdot}2\times10^{-13}$ cm. To interpret this result it is necessary to explain how an α particle can occupy a position of equilibrium at a distance from the centre of the nucleus of the order of 6×10^{-12} cm, the stable portion of the nucleus having a radius of between 2 and 3×10^{-12} cm. Chadwick and Bieler (*Phil. Mag.* Dec. 1921) in their work on the collisions of α particles with hydrogen nuclei have shown that in these collisions the law of force is approximately that of the inverse square outside an oblate spheroid of semi-axes 8×10^{-13} and 4×10^{-13} cm; for lesser distances the law of force changes. Unless at very close distances repulsion changes to attraction it is difficult to see how the nucleus can hold together at all, and to explain Fig. 1 it seems necessary to suppose that the outermost α particles are held to the nucleus by attractive forces in the manner shown in Fig. 2 (*a*) in which electrons are indicated by full circles and α particles by broken circles. The effect of the protons is probably to bind the electrons together, the most stable unit being the α particle. If we adopt this model r will decrease by $2a$ when one electron is emitted. From (6) $a=1{\cdot}85\times10^{-13}$ cm and $2a=3{\cdot}7\times10^{-13}$ cm. Accepting this value, the fact that it is greater than the $2{\cdot}2\times10^{-13}$ cm obtained from Fig. 1 can be accounted for by assuming the nucleus as a whole to rotate with angular velocity ω, in which case the velocity of emission is the resultant of two velocities at right angles to one another, a radial velocity $\sqrt{\frac{4Ne^2}{Mr}}$ and a velocity $r\omega$ of rotation. (The alternative explanation that the electrons suffer distortion under the action of intense forces deserves mention though no method of testing presents itself.) If r is the distance before emission of one α particle from the centre of the nucleus, $(r+2na)$ that of another α particle separated from the first by n electrons

$$V_1^2 = \frac{4N_1e^2}{Mr}$$

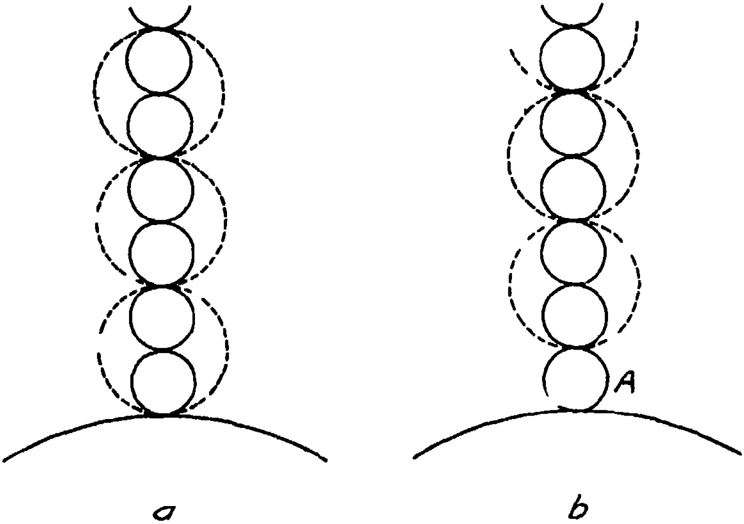
$$V_2^2 = \frac{4N_2e^2}{M(r_1+2na)} + (r_1+2na)^2\omega^2 \qquad (7)$$

By applying these equations to two of the representative substances contained in Table IX, approximate values of r and ω can be found. This has been done for Thorium and Thorium C¹. Substituting the appropriate values for V and N, and noting that $n=14$, it is found that

$$\left.\begin{aligned} \omega &= 1.5\times10^{20}\ \text{sec}^{-1},\ \text{and} \\ \text{for thorium } C_1^1 \quad r &= 2.8\times10^{-12}\ \text{cm} \end{aligned}\right\} \qquad (8)$$

For any other substance in the series

$$r = (2.8\times10^{-12} + 2na)\ \text{cm} \qquad (9)$$



a *b*

Fig. 2

Referring to Fig. 2 (*a*) (which is the simplest possible structure and is not intended to illustrate any particular series) we may imagine an α particle to have approached so near one of the three there shown as to be attracted by and revolve with it. It may then experience a disturbance which will cause it to be released by, say, the third and be attracted by the second. In this way the α particle may move from one circular orbit of radius, $r+2na$ to one of radius $r+2(n-2)a$, in which case an amount of energy

$$\tfrac{1}{2}M\omega^2\left\{(r+2na)^2-(r+2(n-2)a)^2\right\}$$

will be liberated. If, following Bohr, we assume this to be radiated in accordance with Planck's quantum relation,

$$\tfrac{1}{2}Mw^2\{(r+2na)^2-(r+2(n-2)a)^2\}=h\nu \quad . \quad . \quad (10)$$

Substituting for r from (8) and putting $n=2,\ 4\ \ldots\ldots\ 12$ the following values of the wave-lengths are obtained :

·566 × 10^{-9} cm.
·459 ,, ,,
·385 ,, ,,
·333 ,, ,,
·292 ,, ,,
·261 ,, ,,

It seems probable from a study of radioactive changes that the nuclear structure varies to some extent among atoms of the same substance. In Fig. 2 (*b*) is shown a possible variation of the structure illustrated in Fig. 2 (*a*). Proceeding in the same way, the wave-lengths for this arrangement are

·507 × 10^{-9} cm.
·419 ,, ,,
·357 ,, ,,
·311 ,, ,
·275 ,, ,,

Ellis has shown (*Proc. Roy. Soc.*, 1921) that the spectra of the β-rays excited in various substances by the γ-rays from Ra. B can be explained on the assumption that there are present γ-rays of wave-lengths.

·519 × 10^{-9} cm.
·488 ,, ,,
·423 ,, ,,
·354 ,, ,,
·339 ,, ,,
·308 ,, ,,

To sum up, the evidence outlined above suggests that the unstable part of the nucleus of a radioactive atom has a linear structure, and is constituted chiefly of α particles. These are held together by forces of attraction, but if the first particle be displaced sufficiently, the attraction is superseded by the ordinary force of repulsion and the particle is expelled. Towards the end of each series, as can be seen from an examination of the last two columns of Table IX., there are electrons which do not enter into the composition of α particles. and the departure of Ra. F and Th. C from the ordered arrangement shown in Fig. 1. is probably due to the fact that the α particle from each of these substances is liberated in loose combination with an electron, and moves with it for a short distance. The nucleus as a whole is in rapid rotation, and the movement of an α particle one place along the unstable portion involves a change of energy of the same order of magnitude as that associated with the hard γ-rays. This is put forward as a possible origin of the γ-rays. The energy of a γ pulse can be imparted to a β particle and the high-velocity β-rays probably derive their energies from the hard γ-rays, the wide β-ray and γ-ray spectra being due to successive interchanges of energy, each one involving a sufficient expenditure of energy to liberate the electron from the atom.

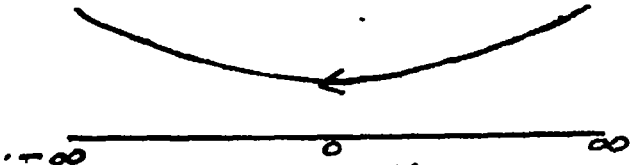
Consider the values of the following integrals, after substituting the values of the constants obtained above, and choosing a suitable value for A_1

$$v_1 = -\frac{Abc^3}{i\pi a_1^3}\int \frac{\sin(ar/c)}{F(a)}\,\frac{e^{-(a_1 a/c)^2 t}}{ra^3}\,da \qquad 11.$$

$$v_2 = -\frac{Abc^3}{i\pi a_1^2}\int \left\{\frac{\sin \mu a(r-c)/c}{\sin \mu a(b-c)/c} + \frac{\sin a \sin \mu a(b-r)/c}{F(a)\sin \mu a(b-c)/c}\right\}\frac{e^{-(a_1 a/c)^2 t}}{r}\,da \qquad 12.$$

where $F(a) = (1/k_2\mu a)\{k_1 a \cos a - k_1 \sin a + k_2 \sin a) \sin \mu a(b-c)/c + k_2\mu a \sin a \cos \mu a(b-c)/c\}$

over the contour suggested by Carslaw (*Phil. Mag*, May, 1920, p. 604); *i.e*, the path P of figure 1, in the a plane, the argument of a on the right lying between 0 and $\pi/4$ and on the left $3\pi/4$ and π.



Path P. Fig 1.

Now v_1 satisfies 1 since each element of the integral satisfies it, similarly v_2 satisfies 2 . Also conditions 4 and 5 are satisfied.

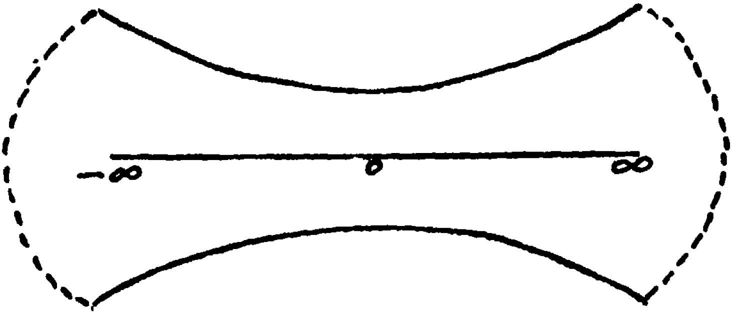
When $r = b$ $\quad v_2 = -\dfrac{Abc^3}{i\pi a_1^3}\displaystyle\int \frac{e^{-(a_1 a/c)^2 t}}{ba^3}\,da.$

Since the integrand is an odd function of a, the path Q (fig. 2) may be formed, consisting of the image of the path P in the real axis, and two circular arcs of infinite radius having the origin as centre.

The value of the integral over path Q is twice that over the path P, since the value over the arcs vanishes in the limit.

The only pole in the contour is $a = 0$ and the residue there is $-(a_1/c)^2 t$.

$$\therefore\ v_2 = At \text{ when } r = b. \quad \text{(Vide 6).}$$



Path Q. Fig 2

$$\text{When } t = 0 \quad v_1 = -\frac{Abc^3}{i\pi a_1^2}\int \frac{\sin(ar/c)}{F(a)\cdot ra^3}\,da.$$

The integrand has no poles above the path P. Hence if the contour is completed by the arc of a circle as in figure 3, the origin being the centre and the radius tending to infinity, the integral over this path is zero In the limit

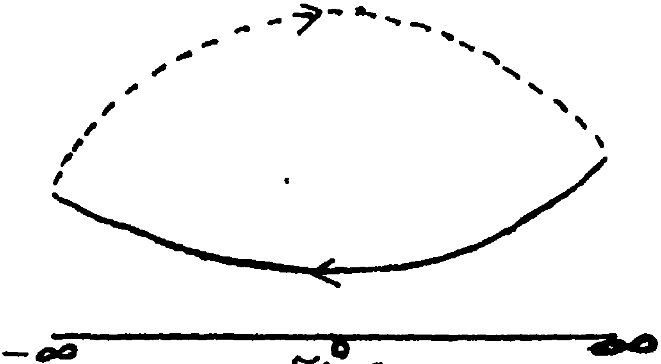


Fig 3.

the value over the circular arc vanishes, therefore the value over the the path P must vanish.

$$\left.\begin{array}{r}\therefore\ v_1 = 0 \text{ when } t = 0\\ \text{Similarly } v_2 = 0 \text{ when } t = 0\end{array}\right\} \quad \text{(Vide 3).}$$

The solutions are :

$$v_1 = -\frac{Abc^3}{2\pi i a_1^3}\int_Q \frac{\sin(ar/c)}{F(a)}\,\frac{e^{-(a_1 a/c)^2 t}}{ra^3}\,da.$$

$$v_2 = -\frac{Abc^3}{2\pi i a_1^3}\int_Q \left\{\frac{\sin \mu a(r-c)/c}{\sin \mu a(b-c)/c}\right.$$

$$\left.+\frac{\sin a \sin \mu a(b-r)/c}{F(a)\sin \mu a(b-c)/c}\right\}\frac{e^{-(a_1 a/c)^2 t}}{ra^3}\,da.$$

the integrands being odd in each case

Considering the value of v_1, we have by taking the residues at the poles of the integrand

$$v_1 = At - \frac{A}{6}\left\{\frac{c^2-r^2}{a_1^2}+\frac{b^2-c^2}{a_2^2}+\frac{2c^3}{k_2}\left(\frac{1}{c}-\frac{1}{b}\right)\left(\frac{k_1}{a_1^2}-\frac{k_2}{a_2^2}\right)\right\}$$

$$-\frac{2Abc^3}{a_1^2}\sum_1^{\infty}\frac{\sin(a_n r/c)}{F^1(a_n)}\cdot\frac{e^{-(a_1 a_n/c)^2 t}}{ra_n^3}-$$

where a_n is the n^{th} root of $F(a) = 0$ and $F^1 = \frac{dF}{da}\cdot$

It can easily be shown that $F(a) = 0$ has an infinite number of real non-repeated roots, there being corresponding equal positive and negative roots.

The mean temperature $= \frac{3}{4\pi c^3}\int_0^c 4\pi r\, v_1 d\gamma$

$$= At - A\left\{\frac{b^3-c^3}{6a_2^2}+\frac{c^2}{15a_1^2}+\frac{c^3}{3k_2}\left(\frac{1}{c}-\frac{1}{b}\right)\left(\frac{k_1}{a_1^2}-\frac{k_2}{a_2^2}\right)\right\}$$

$$-\frac{6Abc}{a_1^2}\Sigma\left\{\frac{\sin a_n}{a_n^2}-\frac{\cos a_n}{a_n}\right\}\frac{e^{-(a_1 a_n/c)^2 t}}{F^1(a_n)a_n^3}\cdot \qquad 13.$$

§ 2.

Same as § 1 with finite conductivity at the surface.

Take $v_1 = -\frac{Ahbc^3}{2a_1^2 i\pi}\int A_1 \sin(ar/c)\frac{e^{-(a_1 a/c)^2 t}}{ra^3}\,d\iota.$

$$v_2 = \frac{Ahbc^3}{2\pi i a_1^2}\int\left\{A_2 \sin \mu a(r-c)/c\right.$$

$$\left.+ B_2 \sin \mu a(b-r)/c\right\}\frac{e^{-(a_1 a/c)^2 t}}{ra^3}\,da.$$

over the path Q since the solutions must satisfy 1 and 2 respectively.

The initial and surface conditions to be satisfied are 3 and 4, together with

$$k_2 \frac{\delta v_2}{\delta r} + h(v_2 - \mathrm{A}t) = 0 \text{ when } r = b$$

where h is the surface conductivity.

This condition leads, after reduction, to

$$\mathrm{A}_2 \sin(\phi - q) - \mathrm{B}_2 \sin\phi = 1/b$$

$$\text{where } \sin\phi = \frac{k_2 \mu a}{bc} \qquad \cos\phi = \frac{k_2 - hb}{b^2}$$

$$q = \mu a(b-c)/c.$$

Using the expressions 9 and 10, giving A_2 and B_2 in terms of A_1, we find

$$\mathrm{A}_1 = \frac{1}{b\left[\sin(\phi - q)\dfrac{k_1}{k_2 \mu a}(a\cos a - \sin a) + \sin a\left\{\dfrac{1}{\mu a}\sin(\phi - q) - \cos(\phi - q)\right\}\right]} = \frac{1}{\mathrm{F}(a)}.$$

$$\text{Hence } v_1 = -\frac{\mathrm{A}hbc^2}{2\pi i a_1^2}\int_{\mathrm{Q}} \frac{\sin(ar/c)}{\mathrm{F}(a)} \frac{e^{-(a_1 a/c)^2 t}}{ra^3}\,da$$

$$\therefore v_1 = \mathrm{A}t - \mathrm{A}\left\{\frac{b^2 - c^2}{6a_2^2} + \frac{c^2 - r^2}{6a_1^2} + \frac{k_1 b}{3ha_1^2} + \frac{c^3}{3k_2}\left(\frac{1}{c} - \frac{1}{b} + \frac{k_2}{hb}\right)\left(\frac{k_1}{a_1^2} - \frac{k_2}{a_2^2}\right)\right\}$$

$$- \frac{2\mathrm{A}hbc^2}{a_1^2}\sum_1^\infty \frac{\sin(a_n r/c)}{\mathrm{F}_1(a_n)} \frac{e^{-(a_1 a_n/c)^2 t}}{\gamma a_n^3}$$

where a_n is the n^{th} root of $\mathrm{F}(a) = 0$.

The mean temperature

$$= \mathrm{A}t - \mathrm{A}\left\{\frac{b^2 - c^2}{6a_2^2} + \frac{c^2}{15a_1^2} + \frac{k_1 b}{3ha_1^2} + \frac{c^3}{3k_2}\left(\frac{1}{c} - \frac{1}{b} + \frac{k_2}{hb}\right)\left(\frac{k_1}{a_1^2} - \frac{k_2}{a_2^2}\right)\right\}$$

$$- \frac{6\mathrm{A}hbc}{a_1^2}\sum_1^\infty \left(\frac{\sin a_n}{a_n^2} - \frac{\cos a_n}{a_n}\right)\frac{e^{-(a_1 a_n/c)^2 t}}{\mathrm{F}^1(a_n)a_n^3} \qquad 14.$$

As $h \longrightarrow \infty$ 14 reduces to 13.

§ 3

Mean temperature of a liquid contained in a cylindrical bulb having infinite conductivity at the surface

The equations to be satisfied by v_1 and v_2 are:—

$$\frac{\delta v_1}{\delta t} = a_1^2\left(\frac{\delta^2 v_1}{\delta r^2} + \frac{1}{r}\ \frac{\delta v_1}{\delta r}\right) 0 < r < c \quad . \quad . \quad . \quad 15.$$

$$\frac{\delta v_2}{\delta t} = a_2^2\left(\frac{\delta^2 v_2}{\delta r} + \frac{1}{r}\ \frac{\delta v_2}{\delta r}\right) c < r < b \quad . \quad . \quad 16.$$

The initial and surface conditions are 3, 4, 5 and 6.

Now $v_1 = \mathrm{A}_1\,\mathrm{J}o(ar)\,e^{-(aa)^2t}$ . . . . . 17.

$$v_2 = \left\{\mathrm{A}_2\,\frac{\mathrm{J}o(\mu ar)}{\mathrm{J}o(\mu ab)} + \mathrm{B}_2\,\frac{\mathrm{Y}o(\mu ar)}{\mathrm{Y}o(\mu ab)}\right\}e^{-(a_1a)^2t} \quad . \quad 18.$$

satisfy 15 and 16 respectively. J*o* and Y*o* are Bessel and Neumann Functions of zero order.

$$\text{At } \gamma = c.\ \ \mathrm{A}_1\mathrm{J}o(ac) = \mathrm{A}_2\{\mathrm{J}o(\mu ac)/\mathrm{J}o(\mu ab)\} + \mathrm{B}_2\{\mathrm{Y}o(\mu ac)/\mathrm{Y}o(\mu ab)\}$$

$$k_1\mathrm{A}_1\mathrm{J}_1(ac) = k_2\mu\{\mathrm{A}_2\mathrm{J}_1(\mu ac)/\mathrm{J}o(\mu ab) + \mathrm{B}_2\mathrm{Y}_1(\mu ac)/\mathrm{Y}o(\mu ab)\}$$

Consider the integrals

$$v_1 = -\frac{\mathrm{A}}{2\pi i a_1^2}\int_{\mathrm{Q}} \frac{\mathrm{A}_1\mathrm{J}o(ar)}{a^3}\cdot e^{-(a_1a)^2t}\,da$$

$$v_2 = -\frac{\mathrm{A}}{2\pi i a_1^2}\int_{\mathrm{Q}}\left\{\mathrm{A}_2\frac{\mathrm{J}o(\mu ar)}{\mathrm{J}o(\mu ab)} + \mathrm{B}_2\frac{\mathrm{Y}o(\mu ar)}{\mathrm{Y}o(\mu ab)}\right\}\frac{e^{-(a_1a)^2t}}{a^3}\,da$$

The condition $v_2 = \mathrm{A}t$ when $r = b$ gives

$$\mathrm{A}_2 + \mathrm{B}_2 = 1.$$

From these equations in A_1 A_2 and B_2 we find

$$\mathrm{A}_1 = \frac{1}{qc[\mathrm{J}o(ac)\{\mathrm{J}_1(qc)\mathrm{Y}_0(qb) - \mathrm{Y}_1(qc)\mathrm{J}o(qb)\}}$$

$$\overline{- \rho\mathrm{J}_1(ac)\{\mathrm{J}o(qc)\mathrm{Y}o(qb) - \mathrm{Y}o(qc)\mathrm{J}o(qb)\}]} = \frac{1}{\mathrm{F}(a)}$$

where $q = \mu a$ $\qquad \rho = k_1/k_2\mu.$

$$\therefore v_1 = -\frac{\mathrm{A}}{2\pi i a_1^2}\int_{\mathrm{Q}}\frac{\mathrm{J}o(ar)}{\mathrm{F}(a)}\ \frac{e^{-(a_1a)^2t}}{a^3}\,da.$$

That v_1 satisfies the condition $v_1 = 0$ when $t = 0$ may be shown as in § 1.

$$\therefore v_2 = \mathrm{A}t - \mathrm{A}\left\{\frac{b^2-c^2}{4a_2^2} + \frac{c^2-r^2}{4a_1^2} + \frac{c^2}{2k_2}\left(\frac{k_1}{a_1^2} - \frac{k_2}{a_2^2}\right)\log_e \frac{b}{c}\right\}$$

$$-\frac{2\mathrm{A}}{a_1^2}\sum_1^{\infty} \frac{\mathrm{J}o(a_n r)}{\mathrm{F}^1(a_n)} \quad \frac{e^{-(a_1 a_n^2)t}}{a_n^2}$$

where a_n is the n^{th} positive root of $\mathrm{F}(a) = 0$.

The roots of $\mathrm{F}(a) = 0$ are infinite in number, real non-repeated and to each positive there is a corresponding equal negative root

$$\text{Mean temperature} = \frac{2}{c^2}\int_0^c rv_1 dr.$$

$$= \mathrm{A}t - \mathrm{A}\left\{\frac{b^2-c^2}{4a_2^2} + \frac{c^2}{8a_1^2} + \frac{c^2}{2k^2}\left(\frac{k_1}{a_1^2} - \frac{k_2}{a_2^2}\right)\log_e \frac{b}{c}\right\}$$

$$-\frac{4\mathrm{A}\cdot}{a_1^2 c_2}\sum_1^{\infty} \frac{c\cdot}{a_n^4} \frac{\mathrm{J}_1(a_n c)}{\mathrm{F}^1(a_n)} e^{-(a_1 a_n)^2 t} \quad . \quad 19.$$

Same as § with finite surface conductivity

Taking v_1 and v_2 as in 17 and 18 and the condition $k_2 \frac{\delta v_2}{\delta \gamma} + h(v_2 - \mathrm{A}t) = 0$ at $r = b$.

$$\mathrm{A}_1 = \frac{1}{qc[\mathrm{J}o(ac)\{\mathrm{NJ}_1(qc) - \mathrm{MY}_1(qc)\} - \rho\mathrm{J}_1(ac)\{\mathrm{NJ}o(qc) - \mathrm{MY}o(qc)\}]} = \frac{1}{\mathrm{F}(a)}$$

where $\mathrm{M} = h\mathrm{J}o(qb) - k_2\mu a \mathrm{J}_1(qb)$

$\mathrm{N} = h\mathrm{Y}o(qb) - k_2\mu a \mathrm{Y}_1(qb)$

$q = \mu a.$

Mean temperature—

$$= \mathrm{A}t - \mathrm{A}\left\{\frac{b^2-c^2}{4a_2^2} + \frac{c^2}{8a_1^2} + \frac{bk_2}{2ha_2^2} + \frac{c^2}{2k_2}\left(\frac{k_1}{a_1^2} - \frac{k_2}{a_2^2}\right)\left(\log_e \frac{b}{c} + \frac{k_2}{hb}\right)\right\}$$

$$-\frac{4\mathrm{A}h\cdot}{a_1^2 c^2}\sum_1^{\infty} \frac{c\cdot}{a_n^4} \frac{\mathrm{J}_1(a_n c)}{\mathrm{F}^1(a_n)} e^{-(a_1 a_n)^2 t} \quad . \quad 20.$$

where a_n is the n^{th} positive root of $\mathrm{F}(a) = 0$.

Numerical conclusions.

The mean lag of the thermometer in all cases takes the general form $\theta_s - \sum_1^{\infty} \theta_n e^{-p a_n^2 t}$ where θ_s is the steady limiting value, θ_n being a function of a_n where a_n is the n^{th} positive root of the appropriate equation in a.

Numerically all terms of the series are negligible compared with the first and thus we may write the lag $= \theta_s - \theta_1 e^{-pa_1 t}$. It is also found that θ_1 differs very little from θ_s and for numerical calculation, the form $\theta_s(1-e^{-pa_1 t})$ will be used.

It is assumed that the wall of the vessel is very thin compared with the radius of the bulb. By this assumption the equation $(F)a = 0$ is very much simplified in order to get the root a_1, which is the first root of the equation other than zero.

In the calculations the units used are *c·g·s*.

To obtain a comparison of the lags in different thermometers, equivalent bulbs are taken, *i.e.*, same volume expansion per degree rise of temperature. Assuming that the coefficient of apparent expansion of alcohol is six times that of mercury, the radii of the equivalent bulbs are as follows :—

	r = radius.	
Mercury sphere	1 cm.	
Alcohol sphere ...	·55 cm.	
Mercury cylinder	·36 cm.	length=10 cms
Alcohol cylinder	·145 cm.	length=10 cms

Also :—

Mercury ...	$k = \cdot 0197$ ..	$a^2 = \cdot 0437$.
Alcohol ...	$k = \cdot 00043$ ...	$a^2 = \cdot 0009026$.
Glass ...	$k = \cdot 0015$ ..	$a^2 = \cdot 00329$.

If the aeroplane is ascending or descending with a velocity of 1,000 feet per minute and the temperature gradient is 1·9°C per 1,000 feet of height, then A = 032°C per second.

The following lags are calculated on the assumption that $b-c = \cdot 05$ cm. in all cases and the experimental result that $h = \cdot 00309$ c.g.s units for a speed of 60 m.p.h.

Infinite surface conductivity $(h \rightarrow \infty)$.

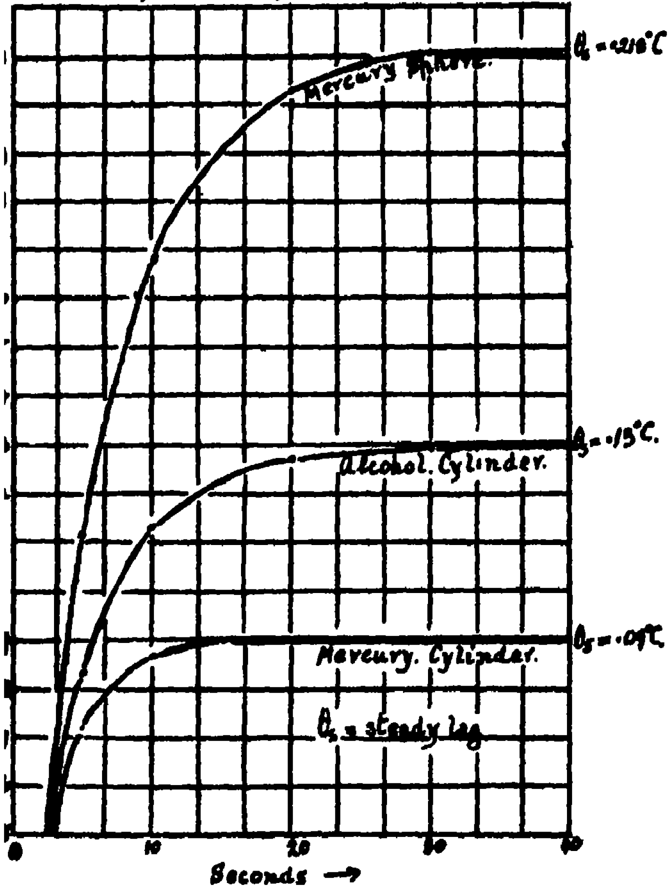
Mercury sphere ...	$\cdot 213(1-e^{-1554t})^{\circ}$
Alcohol sphere ..	$\cdot 81\ (1-e^{-[illegible]t})^{\circ}$
Mercury cylinder ...	$\cdot 09\ (1-e^{-[illegible]t})^{\circ}$
Alcohol cylinder	$\cdot 13\ (1-e^{-[illegible]t})^{\circ}$

Finite surface conductivity (h ·00309 *c.g.s).*

Mercury sphere		..	$1{\cdot}849(1-e^{-{\cdot}02144t})$
Alcohol sphere	...	.	$1{\cdot}79\ (1-e^{-[illegible]})$
Mercury cylinder		..	$1{\cdot}05\ (1\ \ e^{-[illegible]})$
Alcohol cylinder	...	...	${\cdot}60\ (1-e^{-{\cdot}05664t})$

These expressions are shown graphically in the appended diagrams

Infinite Surface Conductivity. Fig 1.



Infinite Surface Conductivity

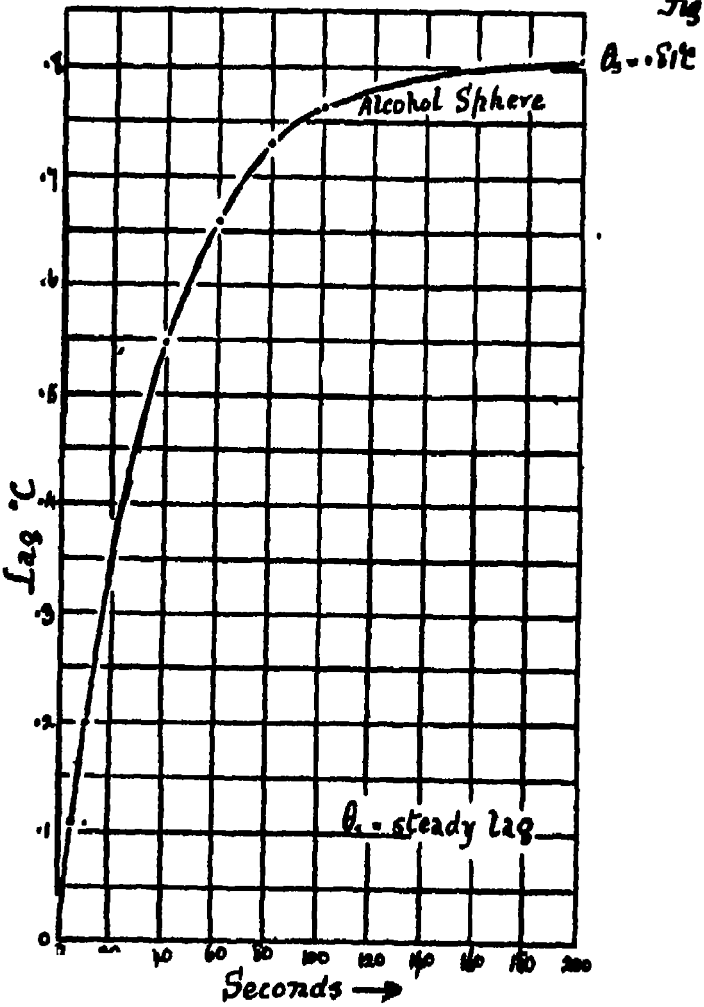


Fig 2.

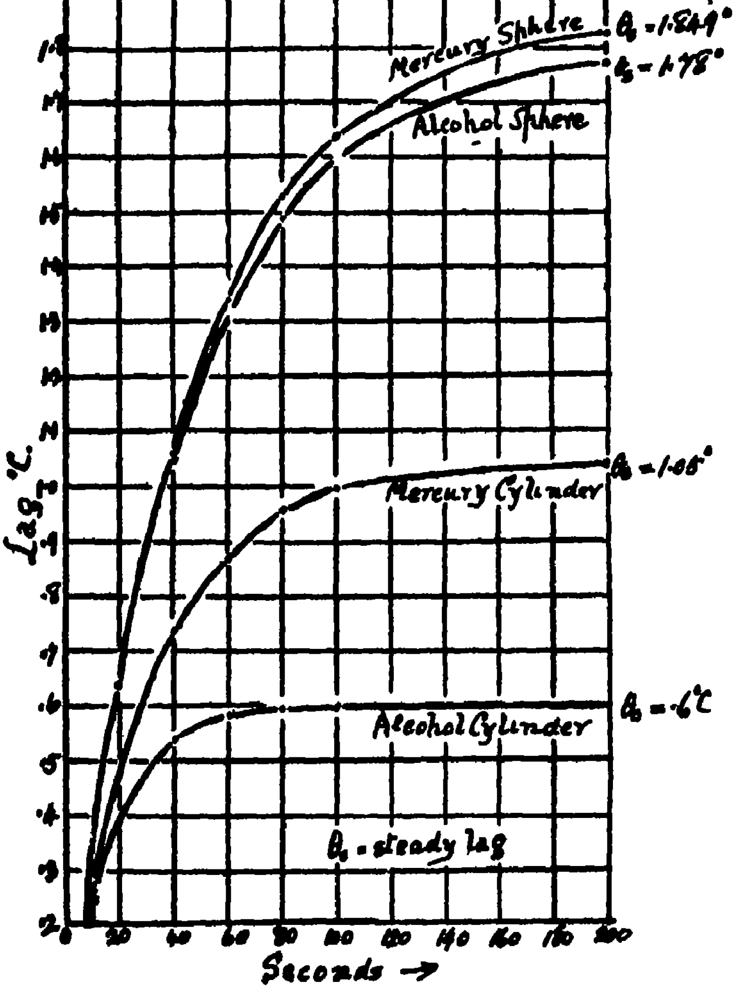


Fig. 3.

Finite Surface Conductivity

THE THEORY OF ROWING.

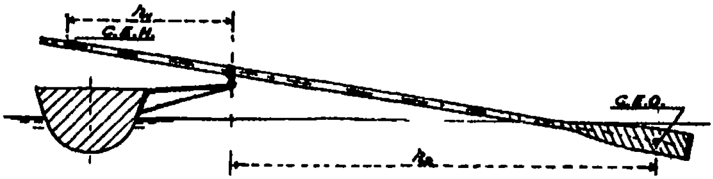
By F. H. Alexander, M.Sc.

Read Nov. 4th, 1921.

Rowing is an art as well as a science. The physiological and psychological characteristics of the rowers put limitations upon the amplitude and duration of the forces they can exert.

Whether these forces are known or not the application of them to the propulsion of the boat comes under the laws of mechanics, and the present paper aims merely at giving a statement of the manner in which these laws operate in the case of rowing; and to explain it by graphic representation.

-Fig I-



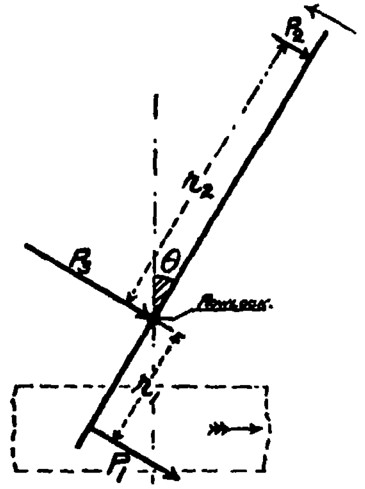
Much has been written about the practical side of oarsmanship; comparatively little about the theoretical side. In 1773 Euler published his "Complete theory of the Construction and Properties of Vessels." A supplementary chapter of this work dealt exclusively with the principles of rowing. He omitted reference to some parts of the problem which are of importance in these days of light boats; but certain later writers might have avoided errors of thought if they had studied that chapter.

Reference will be made later on to some experimental work carried out in France and England in order to ascertain the forces actually employed in rowing.

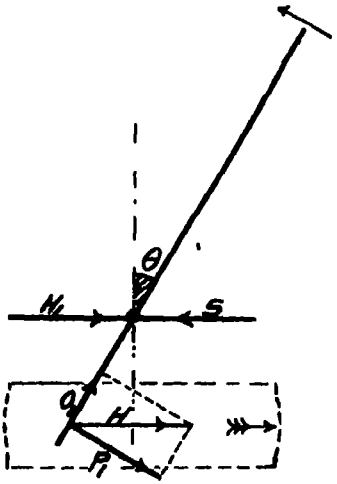
The complete cycle of the rower's movements comprises the "stroke," which propels the boat, and the return or "feather," during which he moves aft to commence the following stroke.

In boats of the racing type, in which the oarsmen sit almost on a level with the water surface, the path of the immersed oar blade is along an arc of a circle forming a section of a cone with its apex at the rowlock. Similarly the hands move along an arc of an inverted conic section. In Fig. 1 C.E.O. represents the "centre of pressure" on the oar blade, and C.E.H. the "centre of pull" of the hands. Strictly speaking, these centres are not fixed points during the stroke, because the outer hand and the outer part of blade are more concerned at the early part of the stroke, and the inner hand and inner part of the blade towards the finish.

— FIG 2 —



— FIG 3 —



The ratio between r_1 and r_2 does not however vary to any considerable extent.

The amplitude of the path of C.E.O. (assuming it fixed) varies with different oarsmen and according to rig and length of oar; but the commencing angle is seldom greater than 50° with the thwartship plane and the finishing angle seldom greater than 40° abaft that plane.

Assuming the blade immersed and making an angle θ with the thwartship plane, consider the principal forces operating:—

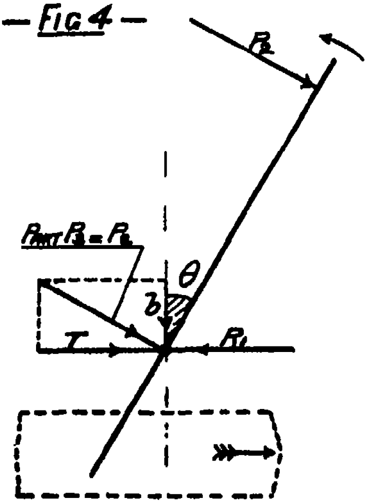
Firstly (Fig. 2) the forces normal to the axis of the oar, are:—P_1 at "C.E.H.": P_2 at "C.E O": P_3 (the sum of P_1 and P_2) at the rowlock.

P_1 and that part of P_3 which is equal to P_1 form an "inboard couple" of radius r_1: P_2 and its equivalent part of P_3 form an "outboard couple" of radius r_2. Taking moments about the rowlock: $P_1r_1 = P_2r_2$.

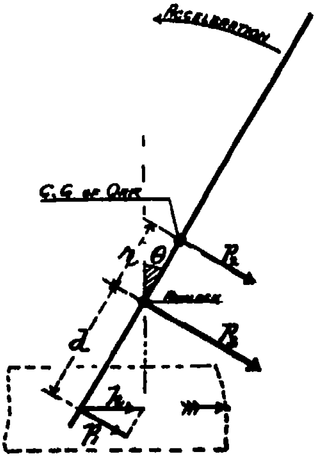
Considering first the "inboard couple" only, H (Fig. 3) represents the fore-and-aft component of the pull of the rower as the movements of his shoulders are more nearly in a straight line than on an arc such as that of the oar handle. The pull H may be resolved into components $P_1 = H \cos \theta$, and $O = H \sin \theta$.

H is balanced by a reaction S of the rower's feet upon the stretcher. This reaction is transmitted by the boat and rigging to the rowlock, and as the rowlock does not move in relation to the boat there must be an equal and opposite force H_1 equal to S.

— FIG 4 —



— FIG 5 —



The force O, acting along the axis of the oar, causes pressure of the button on the rowlock. This force is outward during the earlier part of the stroke, but inward during the latter part. The "inboard couple" does not directly propel the boat, but causes stresses in the hull and rigging.

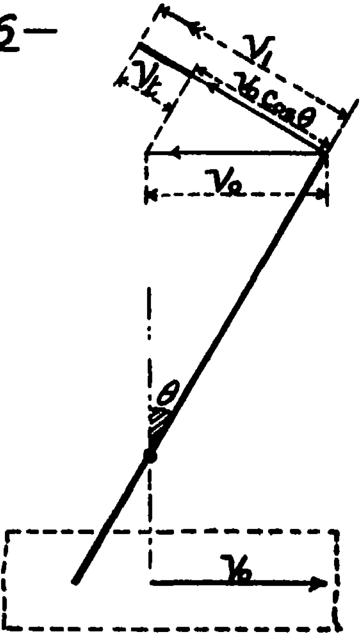
Considering next the "outboard couple," and referring to Fig. 4, the pressure on the oar blade P_2 is transmitted by the oar to the rowlock as part of P_3. This may then be resolved into components $T = P_2 \cos \theta$, and $b = P_2 \sin \theta$. Of these, b acts as an inward compressing force on the rigging during the first part of the stroke and as an outward force during the later part. The force T is that which propels the boat. During the stroke, at any instant, there is a resistance R due to the speed of the boat through the water. At that instant the force (T—R) is available for accelerating the speed. If W represents the weight of the mass to which an acceleration $\frac{d^2S}{dt^2}$ is given, then $\frac{d^2S}{dt^2}=\left(T-R\right)\frac{g}{W}$.

Put into another form, it can be said that $T=R_1$ if R_1 represents the "dynamical" resistance and not merely the resistance due to speed.

We have now considered the principal forces on the oar, but there are certain lesser ones which should not be omitted. At the beginning of the stroke it is necessary to accelerate the angular motion of the oar up to the speed required for its operation; and again at the finish that speed must be checked till the oar is for an instant at rest and about to commence the swing in the opposite direction. This means that the inertia of the oar must be taken into consideration. If $w=$ weight of oar, $k=$radius of gyration, and $r=$distance from rowlock to C.G. of oar, we have :— $pd=\frac{w}{g}(k^2+r^2)\frac{d^2\theta}{dt^2}$ where pd is the couple necessary to give the oar an angular acceleration of $\frac{d^2\theta}{dt^2}$. Thus we have, in effect, a subsidiary oar-like action as shown in Fig. 5

At the C.G. of the oar, at a distance r outside the rowlock, there acts the couple pr, and at the handle the equal couple pd. The hands pull with a component force h during the early part of the stroke and relatively push during the finishing part. During the middle part of the stroke where there may be no acceleration present (as regards oar in relation to boat) these forces vanish. The forces on the rowlock have similar components to those of the principal forces already referred to, but the component corresponding to propulsive thrust at commencement and the component of opposite direction at finish, should not be included with the principal thrust or the principal resistance, because their effect is taken account of when dealing with the effects on speed produced by the movements of the oarsmen and oars, as will be explained later on. It is necessary however to include *h* with H or p_1 with P_1 when considering the actual forces exerted at the hands and on the rowlock, and to distinguish between them in order to ascertain the efficiency of propulsion.

—FIG 6—



We next consider the means by which the pressure P_2 is obtained upon the face of the blade of the oar. If the blade is travelling at the same speed as the water surrounding it the pressures on the front and back are the same, just as a boat floating with the same speed as the current experiences no resistance at either stem or stern. To obtain resistance or pressure on the after face the oar must move faster than the water is moving from it. This excess of speed, which is often referred to as "slip" is in reality the "thrust producing" speed.

If, at a given instant of time, the speed of the boat in relation to the water is V_0 then, at a distance as far out from the boat's side as the oar blade, we may assume the speed of the water passing the boat as V_0 also. The speed of the water away from the blade and normal to the oar axis is $V_0 \cos \theta$ (Fig. 6). If the speed of the oar is V_1 then the excess speed V_t is the speed which causes the pressure P_2. A completely immersed flat plate towed in a direction normal to its surface has been found by experiments to have a resistance expressible in the form $P=K.A.V.^n$ and a value usually accepted for K is 1·2 and for n is 2 where P expresses pressure in lbs. and A area of front surface of plane in square feet, the speed of advance V being in feet per second.

The conditions in the case of an oar blade are not so simple as those of the plane for (1) the surface of the blade is curved, especially towards the tip where it turns towards the direction of motion. (2) The blade is very near the surface, so that an air-filled cavity appears at the back. (3) The motion is transverse to the stream as well as normal to the axis of oar. It is, therefore, possible that the value of K and n may vary during the stroke and may at no part of it have the values given above for an immersed plane. It will be seen later on, however, that the assumption of those values for application to an oar blade appears to give results in reasonable accordance with practice.

The curvature of the blade toward the tip should apparently produce the effect of increasing the pressures near the tip when it first enters the water, and of reducing them near the finish of a stroke. This should cause the centre of pressure first to move outward a little and afterwards to move inward along the blade.

Leaving the subject of propulsion by the oar for a time, we consider next the effect of the rower's movements upon the apparent speed of the boat.

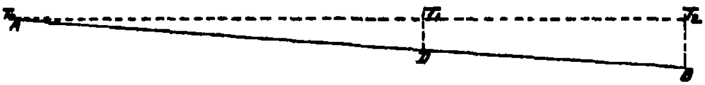
We have a "moving system" consisting of the combined masses of boat, rowers, oars, coxswain, and of a volume of water which accompanies the boat and shares in her changes of velocity. In vessels of ordinary type this has been found by experiment to amount to about 20 per cent. of the displacement.

The movement of the centre of mass of a system is unaffected by movements of parts of that system within itself; but where movements of some parts take place movements of the other parts also take place in an opposed direction so as to maintain the centre of the system in its position relative to external masses.

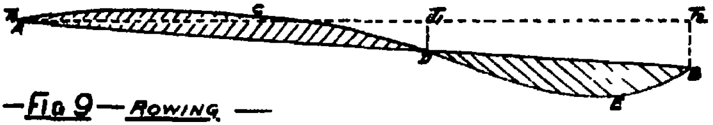
This principle is of importance in the case of light boats with relatively heavy crews: such for example as those used for sculling and racing

In Fig. 7 the time T_0 T_2 represents that occupied in a complete cycle, T_0T_1 representing the "feather" and T_1T_2 the "stroke." If, after completing a stroke, the rowers remain still, the speed falls gradually as represented bv ADB.

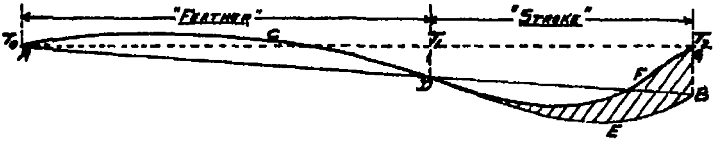
—Fig 7— Rowers Still —



—Fig 8— Rowers Moving: Oars Out —



—Fig 9— Rowing —

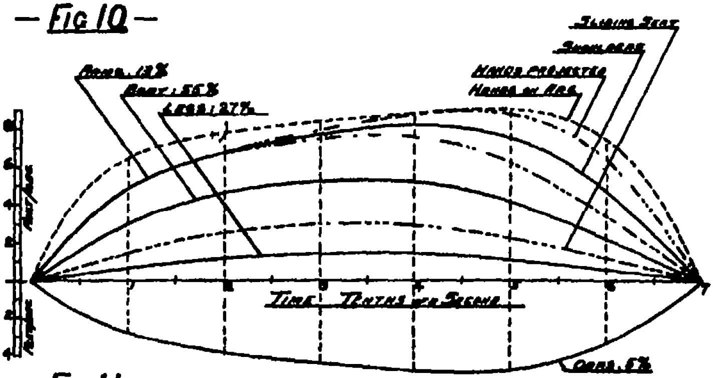


As R varies approximately as $(V_0)^2$ the falling line is not quite straight but curves slightly concave to the time line T_0T_2.

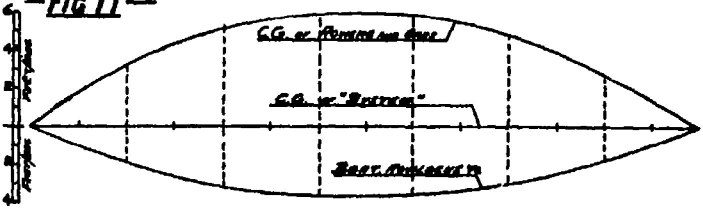
In Fig. 8 the rowers are assumed to move as they would if rowing, but to keep their oars out of the water. The apparent speed of the boat as measured by looking at a fixed part of it (such as the stem-head) is represented by the line ACDEB. If however we could watch the position of the "C.G." of the boat and contents instead of the stem heads, the speed of this "C.G." would still be practically as shown by the line ADB in Fig. 7. It is of course a common experience that when the bow of a small boat has been brought to a bank and one walks forward to get out, the walking forward seems to draw the boat away from the bank.

Now if we consider the shaded area (Fig. 8) between AD and ACD during time T_0T_1 this area represents the distance moved by the boat in a forward direction, and similarly the area between DB and DEB represents the distance moved aft, and these distances must be equal because they result from equal distances moved by the rowers, therefore the effect upon the average speed during the complete cycle is nil. There is however an indirect effect upon propulsion.

— Fig 10 —



-Fig 11-



During the "stroke" between T_1 and T_2 the forward component of blade thrust gives acceleration resulting in a rising curve of speed, which, when superposed upon DEB, gives DFG of Fig. 9. If the speed at G has risen to that of A (at T_0) then the boat is maintaining a "sustained average speed" which may be determined by integrating the speed curve with time, and dividing the distance so obtained by the time interval T_0T_2.

At first sight, the drop in speed during the stroke seems a disadvantage, but it acts in a manner analogous to that of the wake behind a ship upon the screw propeller; it causes a lower speed of oar to get the thrust, and thus prolongs the time during which the acceleration is given; this obviously results in a changing speed V_0 similar to that given in Fig 9 The integral of V_1 gives the movement of the "C E O," and this, converted into angular movement, is represented by the curve θ

The curve $V_0 \cos \theta$ represents the speed of water away from blade, but normal to axis of oar (see Fig 6) Therefore V_t represents the "thrust producing speed"

The instant A is that at which the oar blade should become completely immersed At the instant B it should be brought out

During the time of immersion bending of the oar is caused by the forces at each end, and this causes a slight change of angular position at the blade, the nature of which is represented by the curve marked "θ with lag"

Fig 13 shows the curve of P_2 or blade pressure normal to oar axis corresponding to the accompanying value of V_t From this is derived the component $P_2 \cos \theta$ or propulsive thrust T The integral of this latter curve is proportional to the increase of speed resulting instant by instant, and thus may be obtained the curve V_p (which was referred to in Fig 9 as DFG)

Now the curve of "hands on arc" is directly proportional to that of V_1 in Fig 12, and if the other curves of Fig 10 are obtainable, so as to give the curve V_0 of Fig 12, it becomes possible to adjust the shapes of all the curves concerned so that they fulfil the controlling conditions It is a process of trial, error, and correction if quantitative values are desired

It may help to make the subject clearer to some readers if an attempt is made here to express the various factors concerned by means of estimated quantitative results

The case of a "racing eight" is selected and the following weights are assumed —

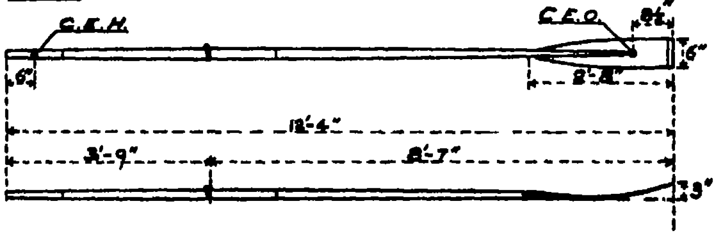
Boat	280 lbs
8 Oarsmen	1,368 „
8 Oars	72 „
Coxswain	130 „
	1,850 lbs
Accompanying water 20 % =	370 „
	2,220 „

The boat is 62 feet long and the resistance at a speed of 17 feet a second is estimated to be 84 lbs. and to vary nearly as $(V_0)^2$ over a range of neighbouring speeds. This resistance includes the effect of the wind pressure caused by the speed when moving in still air. Ninety-four per cent. of the water resistance is due to fluid friction on the immersed surface of the boat. Actual wind against the direction of advance makes a large increase in the resistance, as oarsmen know; it is estimated that a 10-knot wind blowing slightly on one side so as to catch all the rowers and the coxswain, raises the resistance from 84 lbs. to 130 lbs. at a speed of boat of 17 feet per second.

Fig. 14 shows the dimensions of the oar.

The centre of effort of the oar has been calculated on the assumption that the pressures vary from neck to tip as $(V_t)^2$.

-Fig 14-



Some writers make use of the centre of gravity of the blade lamina, but this would involve pressures of the same intensity over the whole surface: others consider the pressure to vary as V_t. Obviously the value of V_t at the neck is smaller than that at the tip. The ratio of "outboard" to "inboard" is taken as 2·4. The arc swept through by "C.E.O." is 12·24 feet in length, the angular movement being from 50° forward of the thwartship plane to 40° aft of it.

Each oarsman is assumed to be of the same force capability as his fellows and to be moving in perfect unison with them; an ideal imaginary crew.

Preliminary trial and correction shows that a maximum pressure on the oar blade of 50 lbs. can maintain a sustained average speed of nearly 17 feet per second and that the time taken in swinging the oar through its full angular distance during the stroke is seven-tenths of a second.

For the 50 lbs. pressure on oar blade the thrust speed V_t at "C.E.O." is 7 feet per second.

Assuming that about 80 per cent. of the weight of each man can be given to acceleration of his own body and that he can in addition use the necessary force to accelerate his oar, it becomes possible to draw the curve of V_1 throughout the stroke. This curve V_1 can not at any instant be more than 7 feet per second in excess of the value of ($V_0 \cos \theta$). The area under V_1 must represent the 12·24 feet swept through by the oar blade. The rate of rise of V_1 at commencement and during early part of immersion must be proportional to the integral of the forces that are employable instant by instant. The integral of V_1 gives the value of θ at each instant and thus $V_0 \cos \theta$ for that instant is obtainable.

The maximum speed of oar is 22·4 feet per second and the "slip" at this speed is 31 per cent.

During "stroke" the initial speed is 16 feet per second; the lowest speed 13·60 feet per second; mean speed 14·80 feet per second; and the distance advanced 10·35 feet. The final speed is 17·56 feet per second.

During "feather" the initial speed must be the 17·56 feet per second just mentioned; the time of fall to 16 feet per second is 1·09 seconds, therefore this is the time allowable for "feather." The maximum speed is 18·96 feet per second; the mean speed 18·10 feet per second; and distance advanced 19·70 feet.

Thus the time of cycle is 1·79 seconds and this means 33½ strokes per minute.

The distance moved during cycle is 30·05 feet giving a sustained mean speed of 16·80 feet per second.

(This speed would, if continued throughout, without allowance for fatigue or for loss of time at starting, and with no wind or current, cover the 6,870 feet of the Henley Royal Course in 6 minutes 50 seconds).

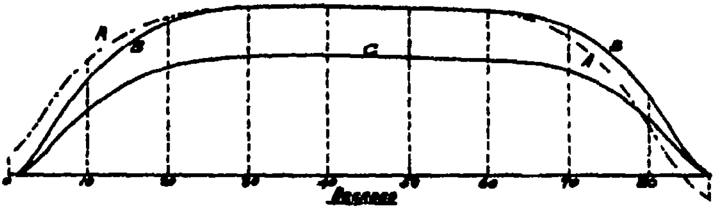
Although the time of stroke is seven-tenths of a second the oar blade is fully immersed for less than six-tenths of a second. Time is required for dropping and lifting and for accelerating and checking the oar.

The maximum force on oar handle normal to axis is 120 lbs. to balance the 50 lbs. on the blade, but at each end of stroke there are the additional forces employed in accelerating and checking the oar, which have been already referred to (Fig. 5).

If these forces are included, it becomes possible to obtain a diagram representing the pressures on the rowlock throughout the stroke, and to separate these pressures into those used in propulsion and those used for acceleration. As the latter are positive at commencement and negative at finish, their final effect upon the propulsive part of the diagram is very small, and appears as a movement of the observed rowlock pressures to a slightly later angular position. In Fig. 15 Curve A represents pressures on rowlock normal to oar axis; Curve B represents the corrected pressures concerned in propulsion alone (the differences being acceleration pressures).

From Curve B may be deduced the Curve C showing the forces P₁ at hands (see Fig. 2), and the area under C then represents the work done at the hands and directly concerned in propulsion. For the illustrative case this work is 500 foot-pounds per man per stroke.

-Fig 15-



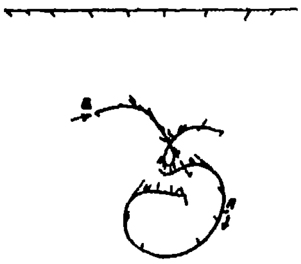
If it is intended to estimate the propulsive efficiency of the rowing, then it is necessary to include the following other pieces of work in addition to the above 500 foot pounds; (*a*) that involved in oar and body accelerations; (*b*) that involved in raising and lowering the C.G. of the body (*c*) that involved in making the whole of the "feather" movement, (*d*) that involved in overcoming internal resistances and subsidiary muscular movements during the complete cycle. Of these, (*a*), (*b*) and (*c*) may be calculated, but (*d*) is at present an unknown quantity, and may vary considerably in different individuals, and with different styles of movement.

The integral of the Speed Curve V_0 (Fig. 12) gives the distance advanced by the rowlock instant by instant, and the integral of

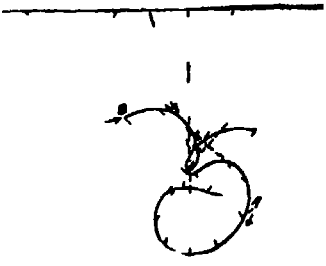
V_1 gives the angular position of the oar so that it becomes possible to obtain a figure giving the path moved through by a point on the oar In Fig 16 A shows the path of the blade tip during stroke (as projected in plan) and B is the path of a point near the neck 3 feet in from the tip Fig 17 represents for comparison a photographic record to which reference is made later on

Now it is reasonable to ask how far are these estimated forces and speeds in accordance with those which actually exist ? To answer this it is necessary to refer to such measurements as have been recorded

-Fig 16-



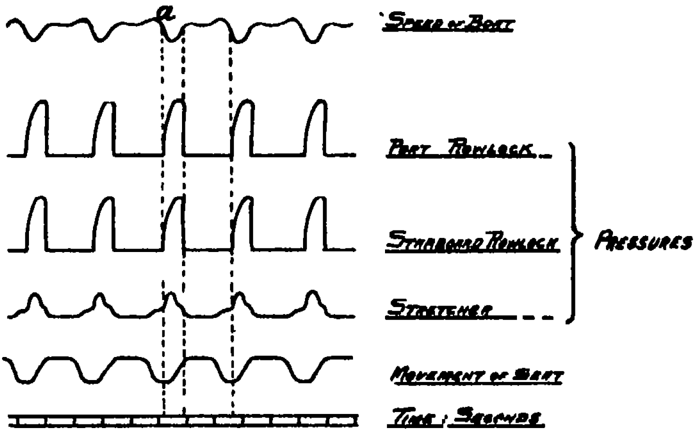
-Fig 17-



In 1904 Mm les docteurs Lefeuvre et Palliotte made measurements of the pressures upon the rowlocks and the stretcher of a sculling boat accompanied by records of the boat s speed and of the movements of the sliding seat The pressures were obtained by the use of air cushions with diaphragms having a small movement and connected by lines of flexible tubing to pens recording upon a revolving drum The speed of boat was taken from a small immersed vane of special shape which recorded pressures assumed proportional to the square of the speed The movements of seat were taken by means of an attached piece of elastic cord of which the diminished stretch near the fixed end was recorded on the revolving drum Thus a straight line represents a stationary seat at each end of stroke The results of these investigations were published in Bulletin de l Association Technique Maritime 1904 under the title Etude graphique du coup d aviron en canoe One of the figures is given here

In the case illustrated by Fig. 18, the sculler purposely rowed with an incorrect style in order to emphasize some of the phenomena which accompany the oarsman's movements. Thus for example the curious sudden rise in speed at (*a*) just before stroke is obviously due to his having sat still during some second or so and then suddenly swung forward to commence the next stroke. Thus the rise in speed which as ACD in Fig. 9 is in the form of a long sweeping curve, is here seen in the form of a short sharp curve. It is interesting to note also that in order to check himself at the finish of this movement he exerts a noticeable pressure on the stretcher just before beginning the severe pressure involved in stroke. The pressure on the two rowlcoks during stroke reached a maximum of 132 lbs. With an assumed oar leverage of 2·3 this would mean 40 lbs. on oar blades and 92 lbs. on hands normal to axis of oar.

—Fig 18—



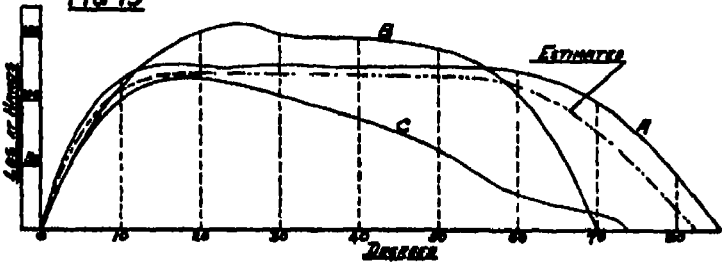
Apparently at start of stroke the pressure on the stretcher reached 220 lbs. Probably most of this was needed for the acceleration of himself and of the two oars.

The scale for speed of boat is not given but the time of oar in water is about five-tenths of a second. The strokes were 26 per minute. Allowing for the shorter oar and shorter arc swept through, this time of stroke corresponds well with the seven-tenths of a second in the imaginary case.

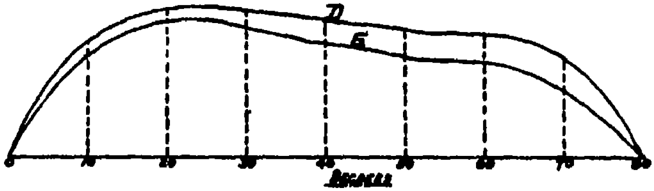
The sudden drop in speed of boat at start of stroke is very clearly marked, but the time scale is so small that the actual shapes of the pressure curves are difficult to determine. This difficulty is increased by the fact that the rowlock pressures were drawn by pens moving radially instead of in straight lines. It is this which causes the curious set forward towards the right at the upper end of these curves.

The investigations included tests of various styles of rowing and the diagrams accompanying the article showed many points of interest which cannot be referred to here for want of time.

-FIG 19-



-FIG 20-



We consider next the records obtained by Mr. E. Cuthbert Atkinson, and published in *Natural Science*, March, 1896, and August, 1898. He designed and fitted an "indicator" on the rowlock, such that a recording pen drew a curve of pressures as they varied throughout the stroke with the angular movement of the loom of the oar. The instrument of 1898 was an improvement upon the earlier one and gave continuous records of successive strokes so that the effect of fatigue could be measured.

Fig. 19 shows three selected specimens of the many records he obtained. As already explained, pressures upon the rowlock can be converted to those at the hands if the leverage ratio of the oar is known, and if due allowance is made for the inertia of the oar at commencement and finish of stroke. The pressures shown are modified from those obtained at the rowlock by using the leverage ratio but no allowance appears to have been made for the effects of oar accelerations. As explained in connection with Fig. 15 the angular positions in relation to actual commencement of stroke would need modification if the diagrams were intended to represent propulsive pressures alone.

In Fig. 19 curve A represents the diagram of one of the most powerful strokes Atkinson recorded. The angular amplitude is very great, and the maximum pressure of 129 lbs. is well maintained throughout its possible range. The work done is 571 foot pounds.

Curve B represents an abnormal stroke of exceedingly high maximum force (166 lbs) but of short amplitude. The work done is 498 foot pounds. and it should be remembered that the time during which the short stroke is in action is lessened in comparison with that of the long stroke, so that this stroke in spite of its high maximum force would be much less effective than A.

Curve C represents a stroke taken upon a fixed seat, and its falling pressures toward the finish are characteristic of all fixed seat diagrams. The work done is 370 foot pounds.

The strokes A, B and C were made when rowing at the rate of 22 per minute.

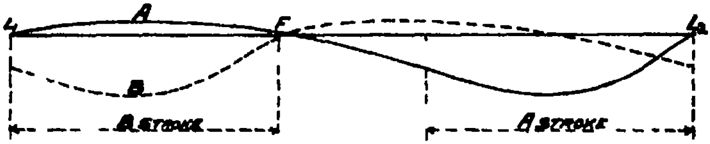
The curve marked "estimated" is deduced from rowlock pressures in the same manner as those of A, B and C and the close agreement in shape between this curve and that marked A shows that the calculations made in this paper are reasonably in accordance with observed facts.

As regards the question of fatigue, Atkinson's 1898 paper gave diagrams to show the falling off in work done after rowing had been in progress for some minutes. It was estimated in a certain case that the fall in 100 strokes was about 13 per cent. and in 150 strokes 18 per cent. Fig. 20 shows an early stroke in pressure diagram D, and E shows the diagram produced 130 strokes later. As the rate of stroke was about 22 per minute, the fatigue effect is that due to about six minutes rowing.

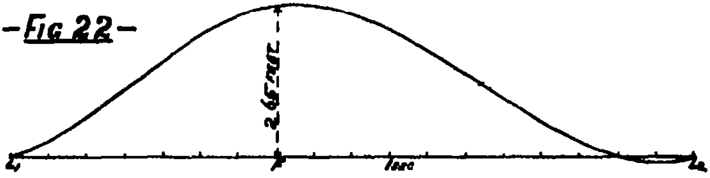
In order to ascertain the movements of the oar blade in the water and the position of a possible stationary point between rowlock and blade tip, Atkinson took a "Cinema" record with 120 exposures at the rate of 14 per second The camera was fixed about 23 feet above the river Cam and used when there was practically no stream to vitiate the results Points were marked on the oar at button, at tip, and at 3 feet in from tip From this camera record the diagram of Fig 17 was made The lower curve is the locus of tip of blade, the looped curve for the point 3 feet in from tip

Comparing this diagram with that shown in Fig 16 the similarity appears to be so marked as to lead to the conclusion that the "slip" used in the calculations of the imaginary case

-Fig 21-



-Fig 22-



is reasonably correct and that the pressure formula $P_s = 1\ 2A(V_t)^2$ may be used without involving serious error

I do not know how much value has hitherto been attached to these investigations of Atkinson, but they seem to me to throw more light on the scientific side of the problem of rowing than any other work I have come across It is however to be regretted that the pressure diagrams were not accompanied by records of the rowers' movements and the speed of the boat The French records would have been greatly increased in value if the diagrams had been more open and increased in scale, and if the scales had been given with them

Many points of interest can be dealt with by these graphic methods, but it would extend this paper to too great a length to refer to them here One point only is illustrated by Figs. 21 and 22.

Fig. 21 shows the speeds of two boats A and B in relation to the speed existing at end of stroke. L_1L_2 represents the time of a complete cycle. Fig. 22 is the integral of the differences of speeds of A and B with respect to time, and shows the gain of distance made by A.

These figures il'ustrate the case of two boats racing together each being similar to that of the imaginary example. They are assumed to have bows level at a distance 13 feet from the finishing line. The crew of boat A have just finished a stroke, but the crew of boat B are about to commence a stroke. Boat A wins the race by 2·65 feet in seven-tenths of a second while boat B is performing the stroke, and advances only 10·35 feet.

Supposing the crew of boat B had sat still and not attempted to row, they would have lost by 2·1 feet instead of 2·65 feet.

Alternatively supposing the crew of boat A had quickened their movement so as to complete the "feather" in seven-tenths of a second instead of in 1·09 seconds, and crew B had rowed the stroke, then A would win by 3·0 feet.

Prominence has here been given to a study of "stroke" only; but during "feather" it is of importance to reduce the work done to a minimum, and with this object, the movement of bodies and oars should have constant velocity over as large a range of the available time as possible, and the accelerations be made short and sharp at each end, as for example by extending the arms at once at the conclusion of stroke.

BLADE LEAKAGE IN REACTION STEAM TURBINES.

(A REPLY TO PROFESSOR CALLENDAR.)

By DR. JOHN MORROW.

[Read December 12th, 1921].

An important factor in determining the efficiency of a reaction steam turbine is the working clearance which must necessarily be allowed between the tips of the blades and the surface of the rotor or cylinder. Since the ordinary test results give the overall efficiency of the turbine, an underestimation of the loss due to leakage through the blade clearances usually leads to exaggerated estimates of the losses due to other causes. The radial clearances vary with the mean diameter of the blade ring, and sometimes also with the length of the blade. Hence these clearances are larger at the lower pressure than at the higher pressure portions of the turbine; but, on account of the shortness of the blades at the high pressure end, the proportion which the clearances when the turbine is hot bears to the blade height is greater there than at any other part. To reduce the clearance losses, Messrs. C. A. Parsons & Co. have during recent years fitted the high pressure portions of their turbines with "end-tightened" blading, and the clearance is then measured in an axial direction between one row and the next as shown in Fig. 1 (*b*), instead of radially as in Fig. 1(*a*). The magnitude of the axial clearances can be varied by means of the adjusting block although, if the turbine be also fitted with axial clearance dummies, the minimum blade clearance, will usually be limited to that which the dummies allow.

If the blading is so arranged that, when the turbine is cold and the rotor pulled forward, the axial blade clearances are zero throughout, then, on heating, and owing to the greater longitudinal expansion of the rotor as compared with the cylinder, the clearances will be smallest at the admission end, that is, at the end nearer the adjusting block, and will gradually increase towards the lower pressure portions of the turbine. A point will be reached at which the axial clearances with end tightened blading will equal and ultimately exceed the radial clearances which would be allowed with blading of the older type. This is, of course, the point at which it is desirable that the end tightened blades should be discontinued.

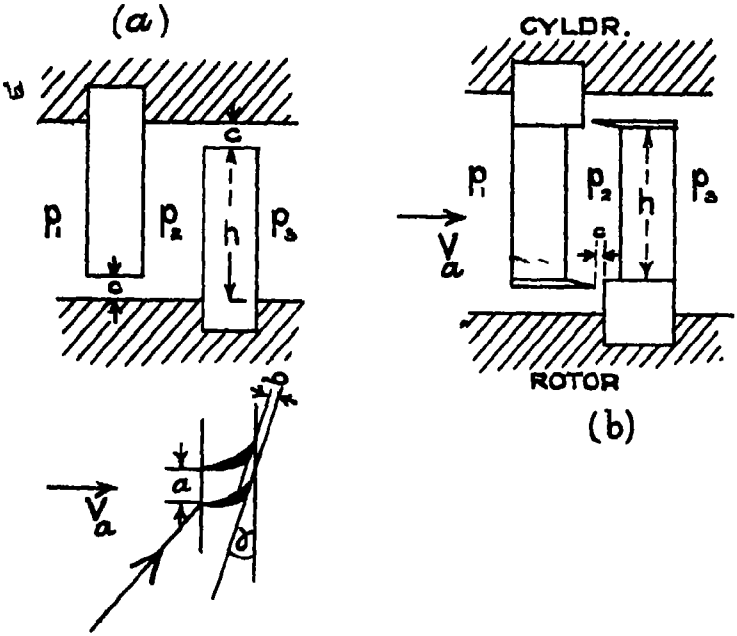


Fig. 1.

In a further development of this method of construction, the clearances in turbines for use on land are arranged to be as small as possible when the turbine is hot and, before stopping, the adjusting block is moved so as to increase the clearances before the turbine cools. Under these circumstances the end tightened blading extends throughout nearly the entire turbine.

When examining, in 1910, the conditions under which reaction blading operated, the author found that the theories of tip leakage were unsatisfactory and he formulated a new theory

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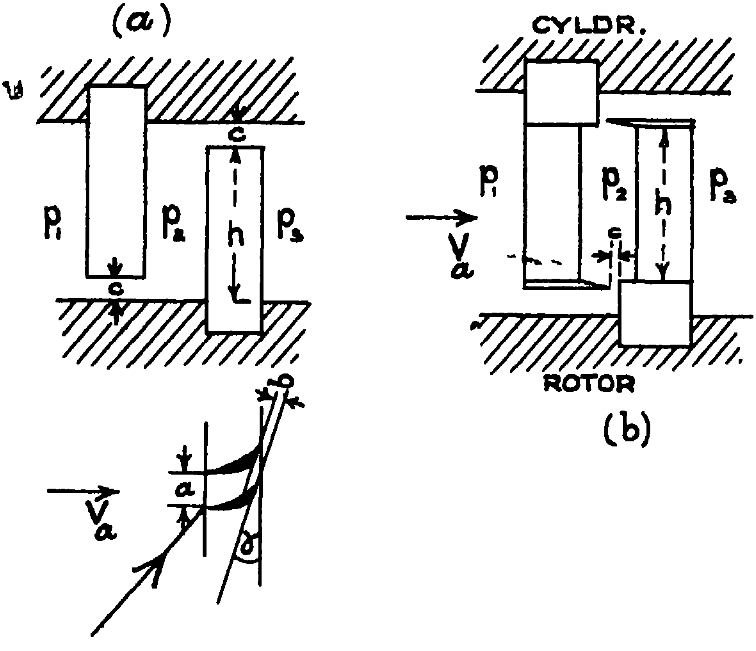

Fig. 1.

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When examining, in 1910, the conditions under which reaction blading operated, the author found that the theories of tip leakage were unsatisfactory and he formulated a new theory to give a better representation of the facts. The desirable features of such a theory, if to be of use in practice, are simplicity in application, a sufficiently close representation of the facts and, as far as possible, a rational basis. A brief account of this was published in 1911 ("Steam Turbine Design," pp. 76-77) and, since it has recently been criticized by Professor Callendar, it is here slightly elaborated, applied to end tightened blading, and verified by comparison with experiment.

Since the clearances are small compared with the blade length, the pressure of the steam in the space between any two rings of blades may be taken to be uniform. Fig. 1*a* shows a stage, comprising one fixed and one moving row, in which the clearances are greatly exaggerated. The steam pressures are denoted by p_1, p_i and p_1. The increase in volume of the steam whilst passing through a single stage is so small that it can be neglected for the present purpose.

Let h=blade height in feet,

c=clearance in feet,

$h+c$=width of annulus occupied by blading,

V_a=axial component of the velocity of the steam whilst entering the blade passages of either the fixed or moving row.

The velocity of the steam when leaving the stationary blades may be denoted by V and we have the relation

$$V_s = kV_a$$

where k is the ratio of the sectional area at inlet to that at outlet, both areas being taken at right-angles to the flow. Usually k may be taken as the ratio of the widths of the channel, that is a/b in Fig. 1: it is sometimes referred to as the area ratio or the gauging factor, and is frequently taken as equal to $\frac{1}{\sin \alpha}$ where α is the blade angle shown on the diagram. Since the heat drop is the same for the clearance steam as for the steam passing between the blades, the magnitude of the velocity generated will be the same, namely kV_a. But in the clearances the steam tends to flow purely in an axial direction with this velocity, whilst in the stationary blades the velocity kV_a is in the direction indicated by the angle α, and the axial component is simply V_a. Assuming that the path of the clearance steam is, to all intents and purposes, axial in direction, it is obvious that the flow of steam through the clearance is k times as much as would flow through an equal length of blade passage.

Thus, considering a sector of blade ring of unit length circumferentially, the steam flowing through the blades of one fixed or one moving row is hV_a cubic feet per second, whilst the steam which escapes the row by flowing through the clearance space is kcV_a cubic feet per second.

Two distinct problems arise: The first is the determination of how much steam flows through an expansion with given clearances; and the second concerns the efficiency factor and may be described as the evaluation of the fraction which represents the proportion of the energy of the total steam which is effective in doing useful work on the moving blades.

Effect of clearances on Steam Consumption.—With regard to the former, it is convenient to compare the actual total steam, W (lbs. per hour, say) with that which would flow through the turbine if the blades remained of the same height but the clearances were supposed to be reduced to zero. This latter quantity may be denoted by W_0.

The ratio of the clearance steam to the blade steam in the actual turbine is kc/h; but owing to the interference with the flow due to the magnitude and direction of the velocity with which the steam issues from the clearances, the average axial component V_a in the blades is slightly less in the actual turbine than it would be if there were no clearances. Assuming that this disturbance is proportional to the relative quantity of clearance steam, we have

$$W = W \quad (1 + fkc/h)$$

where f is less than unity. There is sufficient experimental evidence, both from special tests and from the ordinary trial results of turbines with either radial or axial blade clearances, to show that f is approximately constant and equal to $\frac{1}{2}$.

The total steam may therefore be expressed by the equation

$$W = W_0 (1 + r/2) \quad . \quad . \quad . \quad (1)$$

where r stands for kc/h.

Effect of Radial Clearances on Efficiency.—To obtain a solution of the second problem let us first consider the steam which passes through the clearance space of the fixed row. If it approach the succeeding row with a velocity V_r in the axial direction, its velocity relatively to the moving blades will be in a direction such as that indicated by the arrow in Fig. 1 (*a*). Its kinetic energy will be dissipated in shock and eddies and, as its direction is nearly at right-angles to the reverse edge of the blade, it will interfere with the action of some of the neighbouring steam. Therefore, having a direction of flow which is unsuitable for doing work on the rotor blades, this clearance steam is just as likely to have a brake action as to do useful work. The most reasonable assumption to make, consistent with the simplicity required, is that in the stage in question this steam performs no useful work. Experimental results, described below, justify this assumption.

Hence, of the quantity hV_a which passes through the blade passages of the moving row, kcV_a does no useful work. That is, steam which performs useful work$=(h-kc)V_a$ cubic feet per second, and this is the only steam which passes between the blades of both the stationary and moving rows. It may be referred to as the effective steam.

Since the Total Flow$=(h+kc)V_a$ cubic feet per second, the Efficiency, or the fraction which the effective portion is of the whole, is

$$\frac{h-kc}{h+kc} = \frac{1-kc/h}{1+kc/h} \quad . \quad . \quad . \quad . \quad (2)$$

In other words, for each pound of steam flowing through the turbine, $(h-kc)/(h+kc)$ of a pound does useful work in the stage in question; and the remainder, $2kc/(h+kc)$, is non-effective. The kinetic energy of the non-effective steam, as already mentioned, is dissipated and, increasing the entropy of the steam, it increases also the reheat factor.

It may be noted that the expression for the efficiency is independent of the magnitude of the heat-drop and of the value of the axial component V_a of the velocity of the blade steam. An increase in the clearance usually causes, on account of the additional disturbance which it creates, a slight reduction in the average value of V_a; this however has no influence on the expression for the efficiency as determined by the theory.

The term kc/h, which appears in both numerator and denominator of the expression, is the ratio of the clearance area, c, to the blade outlet area h/k, for unit length of circumference of blade ring. This ratio has already been denoted by r, hence Equation (2) becomes

$$\text{Efficiency} = \frac{1-r}{1+r} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Thus, for example, if $r = \cdot 05$, the clearance area is ·05, or 5 per cent., of the blade outlet area, and the efficiency or ratio of effective to total steam is ·95/1·05, that is ·905 or 90·5 per cent. If, at the same time, the conditions are such that the blade efficiency, assuming no clearance, is 81 per cent., then the efficiency with $r = \cdot 05$ will be

$$\cdot 905 \times \cdot 81 = \cdot 733 \text{ or } 73\cdot 3 \text{ per cent.}$$

The blade efficiency with no clearance depends on several factors, but chiefly on the ratio of the blade speed to the steam speed. When experimenting on the effect of clearance on efficiency it is therefore important that this speed ratio should be kept constant.

In dealing with this subject, Callendar, in his important treatise, "Properties of Steam," (p. 386) states, "Most authorities are agreed in taking the ratio of the working steam to the whole steam as $1-l/x$ to $1+2l/x$, when the annular area factor is 3, for each blade ring whether fixed or moving. But since the leaking steam cannot contribute anything in either case to the impulse or to the reaction, it might appear at first sight as though the loss of efficiency should be doubled." Callendar here appears to admit that the leaking steam is ineffective, but he proceeds as follows: "Morrow (*loc. cit.*, p. 76) takes this view on slightly different grounds, and gives the *effective* proportion of the working steam as being $(1-3l/x)/(1+3l/x)$, which makes the loss of efficiency twice as great as the expression $(1-l/x)/(1+2l/x)$ in the limit when l/x is small, and which leads to improbable results when l/x is large." It will be gathered that Callendar here uses x for the blade height and l for the clearance. The fact that the one theory makes the loss twice as great as the other when the clearances are relatively small is, of course, the point at issue. It was the inability of the older theory to account adequately for the loss that rendered further investigation and replacement necessary. The statement that the newer theory "leads to improbable results when l/x is large" is not in accordance with available experimental data when l/x is limited to such values as occur in practice. Callendar however evidently uses this phrase in reference to values which are many times larger than any which occur in steam turbines, for he continues: "To take an extreme case, when $l=x/2$ . . . . Morrow's fraction would make the efficiency *negative*, whereas it is evident that there would still be some balance of useful work."

With regard to this criticism, which is apparently the deciding factor in Callendar's consideration of the subject, it is only necessary to say that the theory was not formulated to deal with such cases. Callendar accepts as a basis for deduction or calculation (though not necessarily as representing actual conditions) that the quantity of steam passing between the blades can be taken as hV_a and that passing through the clearances as kcV_a. On this assumption more steam, in the special case which he considers, leaks through the clearances than passes between the blades. Some of the fixed blade clearance steam would then pass through the moving blade clearances also, and this leads to the negative sign in the formula.

Further, the statement that there would still be some balance of useful work might be true if the turbine were running at some lower speed conducive to such work being performed; but in reality the speed is determined by the action of all the stages and expansions of the turbine, and it must be assumed to be running at its designed speed and with its designed ratio of blade speed to steam speed; under these circumstances it is probable that the particular stage or expansion with the enormous clearance would act as a brake and justify a negative result from the formula.

Experimental Results.—The effect of clearance on efficiency has been the subject of a very large number of experiments

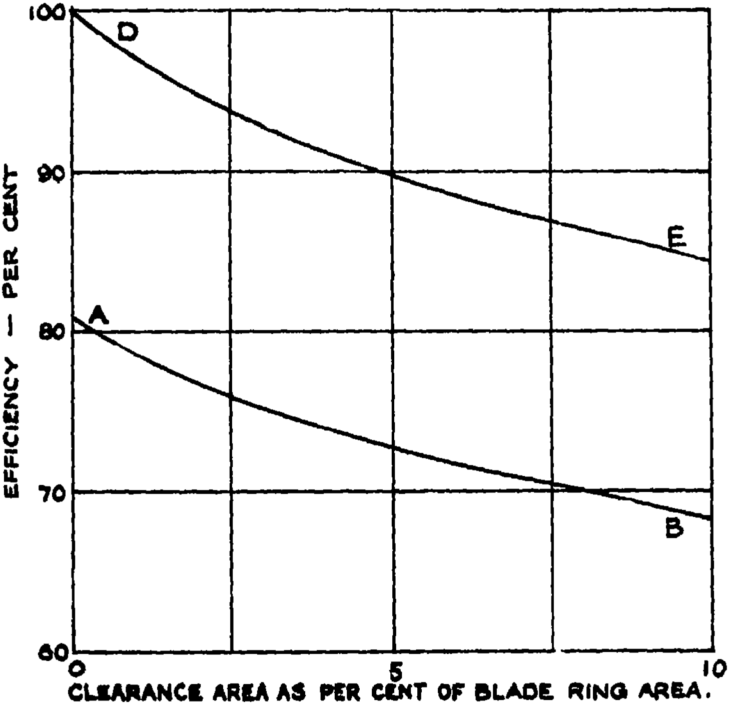

FIG. 2. RADIAL CLEARANCE BLADING.

made by the staff of Messrs. C. A. Parsons & Co. These experiments were carefully carried out and form an exhaustive investigation of the subject. It was found that, for such clearances as occur in practice, the results may be stated in a simple approximate rule as follows: If the clearance area be expressed as p per cent. of the outlet area of the blade channels, then the loss in efficiency due to blade leakage is $2p$ per cent. of the blade efficiency when there is no clearance.

When clearances are excessive the loss appears to be less than given by the above rule, but for all ordinary clearances the rule is in practically complete agreement with equation (2).

By the courtesy of Messrs. C. A. Parsons & Co. I am able to give the following details of a series of tests. They were made on an experimental turbine consisting of a single expansion having seven rows each of fixed and moving blades. Measurements were made of the steam consumption, horse-power, temperatures, pressures, etc., and to ensure accuracy the steam was superheated sufficiently to remain dry throughout the range of expansion. The blades had radial tip clearances and were of the standard $\frac{1}{2}$ inch type of normal blading, with an area ratio of $k=2{\cdot}85$. The clearances were measured with the turbine hot, and these were varied for the different trials, the fixed and moving blade clearances being made as nearly as possible equal. The efficiency ratio, determined from the S.H.P. and steam consumption, is shown by the curve AB in Fig. 2. for a velocity ratio of 0·60 The correction for "re-heat" is very small and may be neglected, and the efficiency for zero clearance is seen to be about 0·81. The curve DE is obtained by dividing the ordinates of AB by 0·81, and from this the loss due to blade leakage may be obtained. The ordinates of DE are thus the efficiency factors or the ratios of the effective to the total steam.

The Parsons' empirical rule, applied to this case, is that at any point on the curve DE, the percentage loss, l, is twice the percentage clearance area p. The results are compared with the various rules in the following table. The last column, headed "axial clearances" has been added to the table and is explained later.

The agreement of equations (2) and (3) with the experimental results is quite good. In order that the figures may be strictly comparable the actual value, 2·85, has been used for the area factor in the formula given by Callendar; it will be seen however that this formula accounts for about one-half only of the observed loss.

TABLE I.—EFFICIENCY FACTORS FOR BLADE LEAKAGE.

r $=kc/h$	CALCULATED FACTORS			OBSERVED FACTORS.	
	Parson's Approx Rule	Equations (2) or (3)	Martin & Callendar	Radial Clearance	Axial Clearance.
0·00	1.00	1·00	1·00	1·00	1·00
·02	·96	·96	·98	·95	·95
·04	·92	·92	·96	·91	·91
·06	·88	89	·94	·89	·88
·08	·84	·85	·93	·86	·85
·10	·80	·82	·91	84	·84

End-tightened Blading.—The consideration given above to radial clearances is applicable also to the axial clearances of end-tightened blading, the only modification being that the flow through the clearance spaces is radial instead of axial. The symbol c now stands for the axial clearance and $r=kc/h$ as before. Equations (1), (2) and (3) may still be used, and we see that the advantage gained by the use of end-tightened blading is not due to any inherent improvement in the efficiency for a given clearance, but rather to the fact that the clearances themselves may be reduced below what is found necessary to ensure safe running with the older type of blade.

It is said that the efficiency, as defined above, may be improved by any amount up to 10 per cent. by the use of end-tightened instead of radial clearance blading ; the actual improvement depends, of course, on the amount by which the clearances can be reduced. It seems not unreasonable to expect an improvement of 5 per cent. in an average case. The following tests on the effect of axial clearance on efficiency ratio were made with end-tightened reaction blading on the experimental turbine at Messrs. Parsons' works :

The turbine comprised 7 pairs of rows of $\frac{3}{8}$ inch end-tightened normal blades having a discharge height of $\frac{7}{8}$ inch, on a mean diameter of $6\frac{7}{8}$ inches. The ratio of mean gauge to mean pitch was 0·37, giving $k=2{\cdot}7$, and the figures in the following table refer to a speed of 3,000 revolutions per minute.

The steam consumption and efficiency ratio are plotted in Fig. 3, and it will be seen that the consumption for no clearance is $W_0=875$ lbs. per hour, the corresponding efficiency ratio being 0·788.

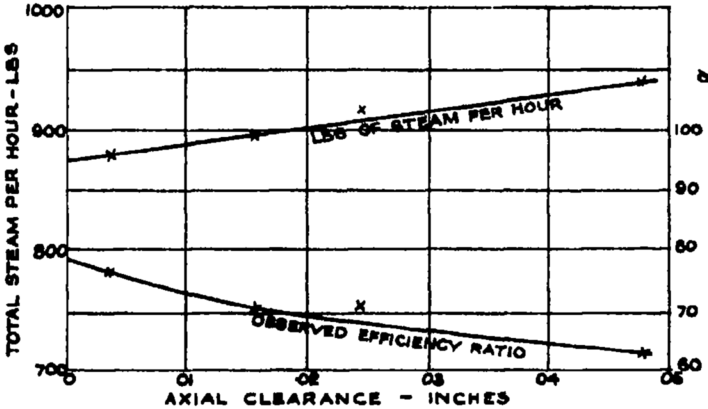

FIG. 3. END-TIGHTENED BLADING.

TABLE II.—TESTS WITH END-TIGHTENED BLADING.

Axial Clearance Inches.	Steam Consumption—lbs./hr.		Observed Effy Ratio
	Observed.	Calculated Eqn (1)	
·0035	880	880	·765
·0158	894	896	·70
·0245	920	908	·71
·0479	940	940	·63

The agreement of the observed steam consumption with that calculated by Equation (1) is very close, the observed value in the third line of the table being obviously on the high side.

The efficiency factors have been calculated from the smoothed curves and are given in the last column of Table I., they agree closely with those obtained from Equations (2) or (3).

The lowest line in Table II. is for an axial clearance considerably beyond the practical limits and the observed efficiency is higher than would be given by the calculation.

The formula adopted by Callendar was originated in the early days of the steam turbine by Mr. H. M. Martin, and it may well have been that, with the larger clearances then in vogue, it represented the facts sufficiently closely. For present day purposes it appears however to be defective, and it assumes apparently that steam which escapes the fixed guide blades is able to perform its full amount of work on the rotor. If this were true, there would appear to be little reason for fitting stationary blades in turbines of Parsons' type

TOWARDS A NEW THEORY OF LAUGHTER.

By J. Y T Greig M.A

[Read February 16th 1922]

It seems to me that the problem of adult human laughter is insoluble, unless it is approached through the simpler laughter of children Almost always—Darwin and Sully are the really notable exceptions in this country—those who have written on laughter have begun at the top and worked only a little way down If they have not all begun and ended with the comedies of Molière as, for instance Bergson has done, they have at the least struck off grand generalizations on laughter, usually in terms of *intellect*, which the merest tyro in psychology can see have no validity for the swift unreflective sometimes almost reflex, laughter of a child * There is no pretending that it is easy to relate the simple laughter of an infant to the complex laughter of an adult but for my own part I would rather frame a hypothesis that will work for the laughter of children, even if it seems not to cover the field with adults, than frame or accept a hypothesis that works for adults and leaves the laughter of children inexplicable

The root idea in modern psychology is force It masquerades under a variety of disguises, calling itself impulse (the English school), the wish (the Freudians), l'élan vital (Bergson), or the libido (the Zurich school) The name is of no consequence for present purposes, and the best way to avoid controversy is to speak clumsily, but guardedly, of psycho-physical energy A hyphen turns away wrath

But the idea of force or energy implies the idea of opposition

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* *Cf* Schopenhauer In everything that excites laughter it must always be possible to show a conception and a particular that is a thing or event which certainly can be subsumed under that conception and therefore thought through it, yet in another and more predominating aspect does not belong to it at all but is strikingly different from everything else that is thought through that conception " *The World as Will and Idea*, Eng Trans , 5th edit , vol ii , p 271

The push outwards of psycho-physical energy, along the broad highways of the instincts, is met by the push inwards of all sorts of other forces. And the push outwards in one direction is opposed or counteracted by the push outwards in other directions. Strains, and stresses, and conflicts of varying degrees of intensity are started, and feeling is the result. Behaviour is felt as pleasant which is in a fair way to succeed, as unpleasant which is being prevented from succeeding. Pleasure and displeasure are both ultimate, neither is conceivable without some degree of the other. And as soon as the feeling of displeasure, which is the feeling of opposition, has ceased to be merely minimal, the complete feeling in behaviour, combining both pleasure and displeasure, has become sufficiently noticeable to be called *emotional*.

Coming down from these abstractions, I turn to the immediate topic. We are fortunate in having a large body of recorded instances of infantile laughter, in the writings of competent observers. In this connection I need name only four—Darwin,* Preyer,† Sully,‡ and Miss Millicent Shinn.§ I have not attempted to supplement their examples in any way, but accept them exactly as they are given. In practically all recorded instances of the earliest laughter of infants there is a common element in the situation which is said to call out the smile or the laugh. This common element is the presence of some second person, almost always of the family circle of course, who attracts the infant's attention to him- or herself. This second person smiles, babbles, sings, nods the head, covers and uncovers the head, or performs some other similar antics, or, more significantly still, touches the child on the lips, or cheeks, or chin.

This may not seem very much to go upon, but I suggest that it indicates that the smile and the laugh, in their beginnings at least, are somehow associated with the instinct of love.

I choose the term "love" with some misgiving, and only because no suitable alternative suggests itself. It must be understood in a very wide sense, as Marshall, for example, understood it,[*] or as the Freudians seem to understand the term "sex" Above all it must unite again the two supposedly separate instincts, sex, and that associated with the tender emotion, which McDougall with a certain mediaevalism of thought has artificially divorced

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* *The Expression of the Emotions*, and *A Biographical Sketch of an Infant.*

† *The Mind of the Child*

‡ *Studies of Childhood*, and *An Essay on Laughter*

§ *Notes on the Development of a Child.*

Touch elicits and touch expresses love, in animals, in children, and in grown men and women As to animals, one need go no farther than the work of Darwin one quotation must suffice Speaking of the desire of cats in an affectionate mood to rub against something, he says, "This manner of expressing affection probably originated through association, as in the case of dogs, from the mother nursing and fondling her young , and perhaps from the young themselves loving each other and playing together Another and very different gesture, expressive of pleasure, has already been described, namely, the curious manner in which young and even old cats, when pleased, alternately protrude their forefeet, with separated toes, as if pushing against and sucking their mother's teats This habit is so far analogous to that of rubbing against something that both apparently are derived from actions performed during the nursing period '[†] For the child, as for the puppy or the kitten, the earliest stimulus of love is the close touch brought about by the nursing embrace, and the instinct is first canalized by way of the lips and cheeks and tongue Infants first "notice" touch on these parts How sensitive the mouth and lips remain ever after in love needs no words of emphasis the kiss is sufficient evidence And along with the kiss should be considered the love-bite At some stage in their development all children tend to fall into this trick , and at times of strong sexual excitement the most civilized of adults tend to "regress" to it From its beginnings, started by the close touch of the nursing embrace, the instinct of love passes over into the other senses The vision of the mother's face, vague though it be, and the sound of her voice, in soothing tones, become "substituted" stimuli And so the child comes to react with love to faces, if well lighted up, and to sounds that are not too harsh in tone The first steps in substitution having been taken, progress is rapid, and may continue almost indefinitely. From the mother who touches and caresses the child, suckles him, sings to him, smiles to him, laughs to him, and is forever disappearing and reappearing, love passes easily to other persons who do the same things, to moving, well-lighted, bumping, sounding, bo-peeping objects, living or dead, to anything associated with such objects, and so to the images or ideas of them.

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[*] "Love is the total vibration of the system which, if carried out to its full conclusion, would make us arise and go to the loved one, as did the prodigal to his father " H R Marshall, *Pain, Pleasure and Aesthetics*, p 78

[†] *The Expression of the Emotions* (Pop. edit , 1904), p 129.

Now it seems to be quite certain that the smile begins in the infant in a quasi-mechanical fashion, as the prolongation of the behaviour of sucking. We therefore find one writer, at least, suggesting that the smile betokens an attitude of the whole organism in which the inception of food is the most striking characteristic.[*] I suggest that we ought to pass on from the primary result of the nursing embrace, the inception of food, to its secondary result, the stimulation of the love instinct. The feeding instinct in the infant is strong and impatient. It is, so to speak, heavily charged behaviour,[*] and all its responses are sharpened to a point, non-contributory movements being out of place. But the smile is just such a non-contributory movement. It gets nowhere, and, in relation to the feeding instinct, is a mere frill. And so it would appear that the smile is dropped out of the very business-like behaviour of feeding, being gathered up and preserved within the much less business-like behaviour of love. For in its beginnings at least, in the infant, the instinct of love is but lightly charged with energy, and its behaviour is loosely co-ordinated, diffuse, easily diverted. In such behaviour the smile hardly gets in the road, because there is no particular road to get in.

It is to be noted that the smile always remains within the behaviour of love, in later life, and only drops out at such times of strong excitement as allow no "frills" at all.

It is more difficult to see how the laugh comes to be added to the smile in the behaviour of the infant. (I assume that the smile does develop into the laugh, and that they are not to be regarded as essentially different phenomena[*]) But I think a modification of the theory first put forward by Herbert Spencer[†] will best serve our purpose here We may suppose an indeterminate amount of psycho-physical energy to be working itself out in love-behaviour, and the smile to be already established, in the way I have suggested, within the ill co-ordinated responses of this instinct Let the behaviour be opposed, or interrupted, it does not matter much how Two courses are now open to the infant He may divert his attention altogether from the end he was striving (not necessarily consciously, of course) to achieve, or he may persist, as against the opposition or obstruction, exerting himself more The respiratory equivalent of such exertion or bracing up is the taking of a deeper breath If the block in behaviour continues, and he cannot overcome it try as he will, his gathering energy will vent itself in gestures which we say "express" displeasure He will cry and squirm perhaps But if, for any reason at all, personal or impersonal, the block or obstruction gives way almost at once, the surplus energy which is no longer required to push against it but which has been "mobilized" for that purpose, may escape in non-contributory gestures similar to the cry and the squirming The smile will carry off some of it, and if this channel is not sufficient, another must be found But it will be remembered that a marked feature of the previous bracing-up was a deeper inspiration This has now to be expired in any case, and the expiration has only to be made a little more explosive and noisy than usual to carry off much of the surplus energy When this has occurred, laughter has been born

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[*] Arthur Allin, in a review of Sully's *Essay on Laughter*, in *Psych. Review*, vol. x., 1903.

[*] It soon ceases to be of this character in civilisation, because we do not wait for children or adults to be really hungry before giving them the next meal!

Analysis of the behaviour of children results in this then, that whenever the laugh is one of the reactions, three elements or moments can be found in the behaviour First, the functioning, more or less, of the love-instinct second, some interruption or check to behaviour and third, the overcoming of the interruption

Whatever there may be new in this theory relates to the first of the three elements or moments. Practically all previous theories of laughter of any value call attention, in one way or another, to the second and third elements or moments. Incongruity, for instance, may be regarded as a logical check or interruption; in so far as it occasions laughter in the person who perceives it, it is a logical check that has been overcome.

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[*] I cannot stay to argue this point, but it seems to me incontestable Exactly the same thing which makes A smile makes B laugh what makes A smile to day makes him laugh to morrow

[†] *The Physiology of Laughter*, in *Essays*, vol. ii Strange as it may seem, Freud's theory of the comic (though not of wit) is closely analogous to Spencer's.

It must be admitted that the functioning of the love instinct, even in the tiny child, may be relatively so unimportant in the whole behaviour which contains the laugh, that for all *practical* purposes it can be neglected. The behaviour even of a small child is not all of a piece; it is a pattern made up of several different strands. The love strand may be subsidiary, but it seems to hold the secret of the laugh.

I have tested out this hypothesis over a wide range of examples of the earliest laughter of children, and it seems to hold. But I confess that I have not yet covered the ground, even hurriedly, with adults, and I therefore hesitate to apply it universally. I surmise that fuller examination will show *either* that a sentiment or disposition of love (at its weakest mere fellow-feeling) can be found, on analysis, to have been stimulated either immediately or mnemically, in the older child, or the adult who laughs; *or*, alternatively, that laughter, as an interruptory gesture, has slipped into the behaviour of other sentiments or dispositions genetically closely related to love. Stendhal perhaps went to the root of the matter when he said, "Il faut que j'accorde un certain degré d'estime à la personne aux dépens de laquelle on prétend me faire rire."* However that may be, it is quite clear that we *laugh* a great deal more, and more heartily, at Falstaff, or Uncle Toby, or Parson Adams, or l'Abbé Coignard, than at Alceste, or l'Avare, or Tartufe; and I suspect the reason is that Shakespeare, Sterne, Fielding, and Anatole France show up the comic, as it were, against a background of affection, whereas Molière is careful at every turn to eliminate the background. Molière inhibits as far as he may his own and our fellow-feeling for his great comic characters, with the consequence that we laugh far less at them than at the rough and tumble heroes of his farces. On the 4th December, 1822, Stendhal kept a careful note of the number of times a Parisian audience laughed during a performance of *Tartufe*. The result was surprising; they laughed only twice. The first occasion was when, in the second act, Orgon, speaking to his daughter Marianne of her marriage with Tartufe, discovers Dorine eavesdropping: the second occasion was provided by a cynical reflexion passed by Dorine on love. There is no separating love and laughter.

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* *Racine et Shakespeare*, ch. 2.

PLATO'S DEVELOPMENT OF THE SOCRATIC PARADOX "THAT VICE IS INVOLUNTARY."

By Miss C M Shipley

[Read Mar. 7th, 1922.]

It is very generally agreed that Socrates was the founder of moral philosophy: for it was Socrates who first asked what was the meaning of those words which men used so glibly—justice, piety, temperance, courage—and discovered the ignorance of those who thought themselves good judges of such matters. His thought was turned in this direction, according to the *Phaedo*[1] of Plato, by an early disillusionment. In his youth, Socrates' imagination had been fired by the investigations of the physicists into nature. He had eagerly hoped that the problems of philosophy were to be solved at last by means of that new datum made known by Anaxagoras—*νοῦς*, mind. Anaxagoras held that mind was the mainspring of the universe; but on analysis it proved to be not so much a mainspring as a makeshift. It was a sort of *deus ex machina*, dragged in to account for physical processes where other explanations failed. So Socrates disappointed in the physicists, betook himself to speculations about the nature of morality, searching for the universal conception apart from the particulars, and inquiring into the motives and ideals of human conduct.

Now Socrates' theory of ethics disregards almost entirely what might be considered a very important element, namely, the will. This is perhaps due to his own peculiarly resolute character, or to the warnings which he received from the divine voice, or to the fact that he was breaking fresh ground in philosophy, and was hampered by the elementary state of psychology—or to all these reasons. But whatever the explanation may be, he believed that sin was involuntary. In the *Protagoras* of Plato, Socrates is represented as saying "I am pretty well assured of this, that no wise man believes that anyone sins voluntarily, or does base and dishonourable actions voluntarily, but they know well that all who do base and dishonourable actions do them involuntarily."[1] There are also two passages in Aristotle which may be quoted here. Aristotle[2] says "Someone may perchance ask how, if a man has a right opinion, he behaves intemperately. For it would be strange if, as Socrates thought, while knowledge was in a man, something else should overcome him and drag him round as if he were a slave. Socrates, indeed, wholly disagreed with the idea on the ground that intemperance did not exist. For no man, he said, understanding what was best, acted contrary to the best, but he did so through ignorance." And again "(Socrates) thought that all the virtues were branches of knowledge, with the result that to know justice was also to be just."[3] It may be gathered from these passages that Socrates ignored the will (whatever we may mean by that) in making up his ethical formulæ. If you *know* what was just, you did it: if you did not do it, that was because you did not *know* what was just. Virtue was knowledge: vice was ignorance. It was a doctrine which revolutionized the conception of conduct and of the nature and function of punishment. It made upon Plato, perhaps the most devoted and certainly the most brilliant of the disciples of Socrates, a profound and enduring impression, and in this paper I hope to show that this novel theory still formed the basis of Plato's conception of morality at the end of his philosophic career, though Plato pondered it more deeply and in some of the later dialogues, especially the *Sophist*, *Timaeus* and *Laws*, gave it an added content and fresh implications by developing it in its place in his own scheme.

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[1] Plato, *Phaedo*, 97c.

In the *Timaeus*[4] Plato, after describing bodily diseases and explaining their causes, turns to the diseases of the soul, which he says are brought about mainly by physiological conditions. The disease to which soul is liable is folly or want of understanding, and this manifests itself in two forms, in madness and in dulness. Owing to certain conditions of the body men are rendered susceptible to feeling excessive pleasures and pains by which they are maddened. Now these bodily affections operate on the soul, making it intractable and impervious to reason. But the real cause of this perversity is not generally recognized. A man who suffers from the corporeal condition which Plato describes (he diagnoses it as an excess of marrow and an abnormally fluid state of that part of the body) is considered not to be sick but voluntarily wicked. But, says Plato, "Almost all lack of self-control in pleasures is caused in this way, and the blame which is bestowed upon them as if men were voluntarily wicked is not rightly bestowed. For no one is voluntarily wicked—κακὸς μεν γὰρ ἑκὼν ουδείς—but it is through some bad state of body and illiberal upbringing that the wicked man becomes wicked, and these are in every case hateful to him and put upon him against his will." Moreover all sorts of failings, peevishness and melancholia, overboldness and cowardice, forgetfulness and want of knowledge are caused by derangements of the bodily system in those regions where the soul is attacked, viz., in the liver, the heart and the brain. Other contributory causes are the influences of badly ordered policies and of false and bad arguments which habituate youth to the form of evil and upon which their souls feed and so become vicious; whereas, as Plato said in the *Republic*, the soul should grow up in wholesome places where sweet breezes may blow upon it and accustom it to communion with the true, the beautiful and the good. For these blemishes in the environment of a man's soul Plato lays the greater share of the blame upon teachers and parents, though, of course, in considering their errors, the same extenuating circumstances may be urged as were urged in the case of their pupils and children. The remedies of these evils are good upbringing and education based on a curriculum containing the right kind of pursuits and studies which Plato describes elsewhere. From this passage it may be gathered that the causes of man's wrongdoing are twofold: (i) an unhealthy and abnormal bodily condition, and (ii) bad teaching and influences. It is tempting, though perhaps misleading, to use modern phraseology and to sum up these causes as heredity and environment.

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[1] Plato, *Protagoras*, 345D. See also *Xen Mem.*, iii. 95

[2] Aristotle, *Eth Nic.*, vii. 3, 1145^b, 21.

[3] Aristotle, *Eth. Eud.*, i. 5, 1216^b, 6.

[4] Plato, *Timaeus*, 86B.

There is a passage in the *Laws*[1] which is equally distinct. "As regards the deeds of those who commit injustice, but whose deeds are curable, we must first realize that no unjust man is voluntarily unjust. For no man would voluntarily possess the greatest of evils, least of all in the most precious parts of himself: and the soul, as we said, is in truth thought by all to be the most precious. In that which is his most precious part, then, no man would voluntarily take, or live his life in possession of, the greatest evil." Plato, in uttering such sentiments in his latest works, showed how well he had learned the teaching of his master Socrates, and how much of that teaching he had adopted at the end, as at the outset, of his philosophic development. His rare humanity with its sense of the dangers which beset man's soul, and the many set-backs to which it is liable in the struggle for goodness, led him like Socrates to look with understanding and sympathy upon the sinner. His belief in the intrinsic value of the beautiful, the true and the good and in the compelling attraction which, when seen, they exercise over the soul, brought him to the conclusion that on the whole man, if he could be made to know what is truly good, would choose it in preference to evil, however enticing. Equipped with a complete knowledge of the good and a healthy body, a man would exhibit unfailing goodness in his conduct. There is some development of the Socratic position. For Socrates the virtues, justice, temperance, holiness, courage, wisdom, were each and all a science, a branch of knowledge, ἐπιστήμη. Plato seems to have made a distinction. Virtue or goodness was the outcome of knowledge. Knowledge was the necessary condition without which virtue or goodness could not be. From such a doctrine, as has already been hinted, there follows naturally a humane theory of punishment. The sinner is in need of healing, training, education. The punishment meted out to him must not be such as to leave his ignorance unenlightened and his disease uncured. He who punishes aright should not be swayed by outbursts of passion and revengeful feelings. Such methods are evil in themselves and in their effects. The lawgiver and the judge must have the spirit of pity dwelling in their hearts, and must show compassion to the ignorant and weak. But if the sinner be incurable (and Plato certainly believed that the wickedness of some men is too great to be cured) then the judge is allowed to be severe. Indignation and anger are the criminal's deserts,[1] and the right and proper display of these emotions argues the possession of a noble nature and a good judgment. For if no training however patient and persevering, and no punishment however wise, can cast out the evil in a man's soul, death is the only fitting penalty.[2] The sinner's life is in reality a thing that is finished, for life to Plato meant growth in the light of knowledge, and the thing that is wholly evil abides in the darkness of ignorance and stagnation. Such a man should for his own sake and for the sake of his fellow men be helped out of an existence in which there is no hope of his profiting either himself or others. Some of the most hardened sinners cannot be corrected even by the penalties inflicted after death, according to the account in the *Republic*, but are finally cast into Tartarus from which there is no return.[3]

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[1] Plato, *Laws*, 731D.

There is an interesting passage in the *Sophist*[4] which should be compared with the account of the failings and frailties of the soul given in the *Timaeus*. In this dialogue the speakers are engaged in the search for the sophist, and in the course of the hunt the talk comes round to the subject of purification. The Eleatic stranger, the chief speaker in the dialogue, analyses purification and finds that it is of two kinds purification of the body and purification of the soul Now to purify the soul means to purge the soul of its evil, and there are two sorts of evil in the soul One of these is called πονηρια, wickedness, which the stranger compares with νοσος, disease in the body, and στασις, strife or discord, and the other sort of evil is ἄγνοια, ignorance, comparable with αἶσχος, ugliness in the body—an example of ἀμετρια, lack of proportion [1] Theaetetus, who is talking with the stranger agrees with this classification "I must entirely concede what I doubted when you spoke of it just now, that there are two kinds of evil in the soul, and that we ought to think that cowardice, lack of self-control, and injustice are all a disease in us, and that we should define ignorance, various and manifold as it is, as ugliness"[2] These forms of evil are cured respectively by ἡ κολαστικη τεχνη and ἡ διδαστικη τεχνη, the art of correction and the art of instruction the art of correction being compared with the art of medicine and the art of instruction with the art of gymnastic The analogy between the soul and the body is therefore used consistently throughout this part of the discourse Plato stresses the analogy between the defects of the soul and the defects of the body, and this analogy is still further developed when he goes on to consider the remedies employed in the treatment of these respective sorts of defect Now ignorance and the instruction which can enlighten it are again subjected by the stranger to the process of division One branch is very large in comparison with the rest—Plato calls it difficult [3] This sort of ignorance is unwarranted conceit of knowledge, and its name ἀμαθια, dulness, stupidity, senselessness is that which is given in the *Timaeus* to one of the forms of evil in the soul The remedy for this is παιδεια, education, not the sort of rebuke and admonition so often used sometimes in an angry, sometimes in a gentler form of remonstrance, but the painstaking persuasive method of dialectic which by right reasoning shows a man where he is ignorant and biassed. So it turns his conceit of knowledge into modesty and predisposes him towards the reform of his unhappy state.

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[1] *Laws*, 731D.

[2] *Laws*, 854E

[3] *Republic*, 615E.

[4] *Sophist*, 227D, sqq.

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[1] For the view that vice is disease and ugliness, see *Republic*, 444E

[2] *Sophist*, 229C

[3] *Sophist*, 228E

At first sight the analysis of evil in the soul which Plato makes in the *Sophist* does not seem to fit the account given in the *Timaeus*. One part of ἄγνοια, ignorance, a very large and important part, is in the *Sophist* declared to be 'αμαθία, stupidity, which, as we saw, is the word used in the *Timaeus* to name one form of badness in the soul. That great subdivision of vice in the soul which is called in the *Sophist* πονηρία, wickedness, does not fall into the scheme of the *Timaeus* so easily. It manifests itself in various ways: injustice, cowardice, licentiousness and insolence are forms of it. In some respects this class corresponds to the class called μανία, madness in the *Timaeus*, where indeed the distinction between the two kinds of evil is not always kept very clear. Madness, according to the statement in the *Timaeus*, is directly brought about by abnormal and defective bodily conditions which make men indiscriminate and over-eager in their choice of pleasures and avoidance of pains. The man who is too sensitive in regard to pleasure and pain, and who pursues the one and avoids the other unduly, is mad. "He is able neither to see nor to hear aught rightly: he raves, and at that time is quite incapable of partaking of reasoning." But excessive indulgence in pleasure is intemperance, and an inordinate fear of pain is cowardice. Insolence, ὕβρις, is a form of intemperance. All three, as Plato himself says in the Republic, are manifestations of injustice. That wickedness, then, which is described in the *Sophist* as revealing itself in intemperance, cowardice, insolence, and injustice, is very similar to the madness of which we hear in the *Timaeus*. The approximation of the two classes is further emphasized by the fact that wickedness in the *Sophist* is looked upon as being a sort of disease in the soul while the madness of the *Timaeus* is also considered to be one of the soul's diseases, the cause of which Plato finds to reside in a great measure in an unhealthy bodily condition. Thus it will be seen that the forms of evil in the soul mentioned in the *Timaeus* are to be found in the analysis which is worked out in the *Sophist*. It is not essential that the two classifications should tally exactly in every detail—the really important point to observe is the unity of idea and purpose which underlies these accounts.

In the passage taken from the *Timaeus* there is an intriguing development. Plato attaches a remarkable importance to the body as a cause of error and wrong-doing. An ill-conditioned body makes a man bad-tempered, causes him to react wrongly to pleasures and pains, and blunts those delicate spiritual and intellectual faculties the perfect exercise of which can alone ensure goodness. Plato had never before stated this theory with such impressiveness and conviction, though the idea was not altogether new. He speaks in the *Phaedo*[1] of the tiresome necessity of nourishing the body and of the waste which illness causes of time that might have been spent in seeking after truth. The Pythagoreans regarded the body as a tomb, σῶμα . . . . σῆμα: that was one of the dogmas of their religion, an expression of their mystic faith. Socrates, too, apparently allotted to the body a large share of the blame for moral and intellectual failings. Xenophon[2] tells us that he considered ill-health to be a source of errors in reasoning, while it also made a man melancholy and mad. This doctrine is further evolved in the *Timaeus* and its bearing upon human responsibility is clearly stated. The relation of this theory to Plato's metaphysic can be inferred from this dialogue and should not be entirely passed over, though any examination of the many problems involved is beyond the scope of the argument. It would seem, however, from the exposition in the *Timaeus* that the individual does and can only exist on conditions which involve the imperfections of his existence. For the universal soul, which creates the world, creates it by evolving itself into a plurality of souls and by so evolving itself submits inevitably to the limitations of time and space. But limitation means imperfection and departure from type—hence arises evil. The soul of the individual living creature is affected by the imperfect body, and becomes diseased by its implication in the body's limitations.[1] We have already seen what those special diseases are to which the soul is subject.

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[1] *Phaedo*, 66B.

[2] *Xen. Mem.*, iii, 12, vi.

It has been shown that Plato, in the later part of his life perhaps more than in the earlier, considered that bodily infirmities and bad environment were accountable for the misdeeds and evil habits of men, and it now remains to discover whether he admitted human responsibility for sin to any extent or whether he was a thorough-going determinist. There is that very clear statement in the *Timaeus*, κακὸς μὲν γὰρ ἑκὼν οὐδεὶς, no one is willingly wicked, supported by the prouncement in the *Laws* πᾶς δ'ἄδικος οὐχ ἑκὼν ἄδικος, no unjust man is voluntarily unjust. And in the *Sophist* when speaking of the distinction between wickedness and ignorance, he says "There is too that other evil which they call ignorance but which they are unwilling to admit to be vice, though that is the only 'vice' that comes into existence in the soul."[2] When we remember that a little while before the stranger forced Theaetetus to confess that no soul is voluntarily ignorant, we may arrive here at the conclusion which is so emphatically stated in the *Timaeus* and the *Laws*, that no one is voluntarily vicious. It is possible, however, to translate as Jowett does "And there is that other (evil) which they call ignorance, and which, because existing only in the soul, they will not allow to be a vice." But apart from this, Plato clearly identifies himself in the *Sophist* with that class of instructor of whom the Stranger speaks a little further on when he says that some people seem to have concluded, after due thought, that all dulness is involuntary.[1] Plato does not say here "that all *evil* in the soul is involuntary," but chooses to predicate involuntariness of only one of the soul's *evils* because no wider statement is needed for the particular problem with which he is dealing. He is tracking the *Sophist* down, by the method of division, through that part of ignorance which is called stupidity, dulness, absurd self-conceit: this is said to be involuntary, and for the purposes of the discussion it is not necessary to call the other forms of evil involuntary, though they may be, and, on Plato's view, almost invariably are involuntary. Hence there is nothing in this passage of the *Sophist* which qualifies the statement in the *Timaeus* and the *Laws*.

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[1] For this view see Archer Hind's introduction to his edition of the *Timaeus*.

[2] *Sophist*, 229D.

Such, then, is the doctrine to which Plato gave his adherence. There are, however, a few passages in the dialogues that might be thought to admit of an interpretation not altogether consistent with the theory which I have outlined. Let us turn first to the story of Leontius and his fight with desire, and follow whither the argument leads us. Socrates tells the tale in the *Republic*. "I once heard a story which I believe to be true that Leontius, the son of Aglaion, was coming up from the Peiraeus and drawing nigh to the northern wall on the outside he caught sight of some dead bodies lying there, with the executioner hard by. He was eager to look at them and at the same time he hated the idea and tried to turn his mind from it. For a long time he fought against it and covered his eyes, but at last, overcome by the desire he pulled his eyes open, ran up to the corpses, and said 'There now, you wretched things, glut yourselves with the lovely sight.'"[2] Socrates had been examining those three principles in the soul—the rational principle, the spirited principle, and the concupiscent principle, and the story illustrates the conquest of that part of the soul which is spirited and indignant at unworthy actions by the part which desires: it also serves to show that these two parts are distinct. Socrates goes on to say that this indignation frequently fights on the side of reason against the desires. Now if reason tells a man what is right, and indignation, which is a sort of conscience, is fighting along with reason against a base desire, and if a man in spite of reason and conscience does what his desire prompts him to do, can he be said to have acted involuntarily? To this question Plato would probably have replied "Yes." The feeling displayed on this occasion is a stupid curiosity, a mixture of senselessness and intemperance. The action is tainted with madness, to use the categories of the *Timaeus*. At the moment when Leontius cast out the dictates of reason and decency and was carried away by the desire for an unworthy excitement, he was temporarily mad; "he is able neither to see nor to hear aught rightly; he raves, and at that time is quite unable to partake of reasoning." Now madness, as we saw, can be traced to an unhealthy state of body, possibly fostered by lack of good training. It was not then Leontius' fault, Plato would have said; he was relieved of the responsibility for his action. And since his baseness is shown to be involuntary, Plato is not inconsistent.

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[1] *Sophist*, 230A.

[2] *Republic*, 439E.

Take again the famous passage which occurs at the end of the *Republic* in the myth of Er.[1] In this Er tells how the souls, after they had finished the journey of a thousand years which awaited them after death, came at last to Lachesis, on whose knees were lots and models of lives: and the interpreter who had marshalled the pilgrims announced that the time had come for them to choose a mode of existence and begin life on earth again. "A destiny shall not choose you," he said, "but you shall choose a destiny. Let him that draws the first lot choose a life which shall be added to him of necessity. But virtue knows no master. He that honoureth her shall have more of her, and he that dishonoureth her shall have less. The responsibility rests with him who chooses: God is not responsible." So they drew lots, and then the various samples of lives were exhibited. The lots outnumbered the souls who were present, and there were lots of every kind of human and animal life. It was indeed a fateful moment in the souls' existence, and Socrates pauses a moment in the story to mark the gravity of the choice, and to give a short exhortation on the necessity for expending every effort to fit the soul for that supreme crisis of its fate. The souls were assured that even the last comer, if wisdom were exercized and the soul were not lazy in its next life, had the opportunity of making a good choice; while a good choice was not guaranteed even to the holder of the first lot, unless he took care. It is interesting to note that one of the souls who had led a blameless life in his previous existence made a disastrous selection, choosing the life of a tyrant because of its power and riches, and not seeing in his greed and heedlessness that it would bring upon him terrible calamities. The reason of his folly was that in his former life his lot had been cast in a well governed state, and his virtue had been only a mechanical thing, produced by forces from without himself not in his own heart. His virtue had not been based upon reasoned principles, and when temptation came he fell through ignorance. The souls then all made their choice, and after having passed through the Plain of Forgetfulness and having drunk of the River of Unmindfulness, they rested. They were roused from their repose by a loud clap of thunder and an earthquake, and were reborn, " shooting up," as Plato says, " like stars," to lead the lives which they themselves had chosen.

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[1] *Republic*, 614B.

The difficulty in this passage lies in the words of the interpreter, " He who chooses is responsible: God is not responsible." If the soul when it made its selection of its future life was to be held accountable for its choice, then we must suppose that it had a real choice, that it could in some sense have chosen otherwise, and that it had a measure of free will. But hitherto our attention has been concentrated rather on Plato's determinism; we have disregarded the element of freedom. Leaving this passage for a moment let us turn to a statement in the *Laws* which introduces a similar problem. " In the making of the whole God contrived an arrangement of the various parts which would best and most easily secure the victory of the good and the defeat of evil. Now he has devised the kind of place and abode which each particular character must share and inhabit in relation to the whole. But he left to the wishes of each of us the formation of the particular character. For the quality and character of almost all of us nearly always depend upon the direction of our desires and the nature of our souls."[1] (I do not think that this passage gives us any fresh data: but it confirms the theory which was stated in the *Republic*. It is, in fact, a reiteration of the blamelessness of God and the responsibility of man, not in the choice of a future life, but in the choice of our character in this present existence, in the making of which our desires and dispositions play a great part.

We are now in a position to ask what Plato's attitude really was to the contending claims of freewill and determinism. Or since the modern terms freewill and determinism have a wider and more complex connotation than is suitable for our purpose, and the problem presented itself to Plato in a simpler form, let us put the matter differently, and ask " Did Plato hold men responsible for their actions? Did he think that men are free to choose what they will do or be? " Plato's answer to these questions whether we find it satisfactory or not is all the while " tumbling about before our feet." The interpreter in the myth of Er warns and encourages the souls who are about to make their choice by proclaiming to them that virtue has no master, ἀρετὴ δὲ ἀδέσποτον, Proclus interprets the words as meaning " virtue is not only in our power, but it frees us from many harsh masters, slavery to whom deprives us of all good things." There is, then, for Plato some sense in which men are free to choose. Virtue waits for them to take her. Let them only regulate their desires aright and she is their's. But Plato is loath to blame when the choice goes wrong. There are many evil influences that constrain a man unwillingly to do wrong—a bad bodily condition and bad training are especially potent for ill.

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[1] *Laws*, 904C.

So Plato does not solve the problem—he states it anew. Man is not without responsibility, for though vice is involuntary, virtue is free. For this reason Plato is ever stressing the need to seek diligently after the good. There is still a place for enthusiasm and endeavour, and though the hindrances to man's efforts to attain to virtue are great, the task is not impossible. He must lay hold of the golden chord of reason, of the laws of God in the world, the city, and in his own soul, and if the work be well done, he may escape at last from the ills and wickedness of this world to dwell apart in his own particular star.

University of Durham Philosophical Society 1919-21

Revenue Account

	£ s d			£ s d
To Subscriptions	69 15 0	By Stationery and Circulars		8 19 0
Armstrong College Library Committee	1 0 0	Lecture Assistant		0 5 0
Sale of Proceedings	0 9 0	Clerical Work		12 4 7
Overprints &c	4 9 6½	Postage		6 6 6½
Interest	5 19 6	Printing Proceedings		
		Vol. VI Part and Index to Vol. V	£35 12 8	
		Overprints	3 2 6	
				38 15 2
		Surplus from November 21st, 1919 to October 19th 1921		34 1 9
	£100 12 0½			£100 12 0½

Reserve Fund

	£ s d		£ s d
To Balance forward	57 0 0	By Balance at October 19th 1921	100 0 0
Transfer from Revenue Account	43 0 0		
	£100 0 0		£100 0 0

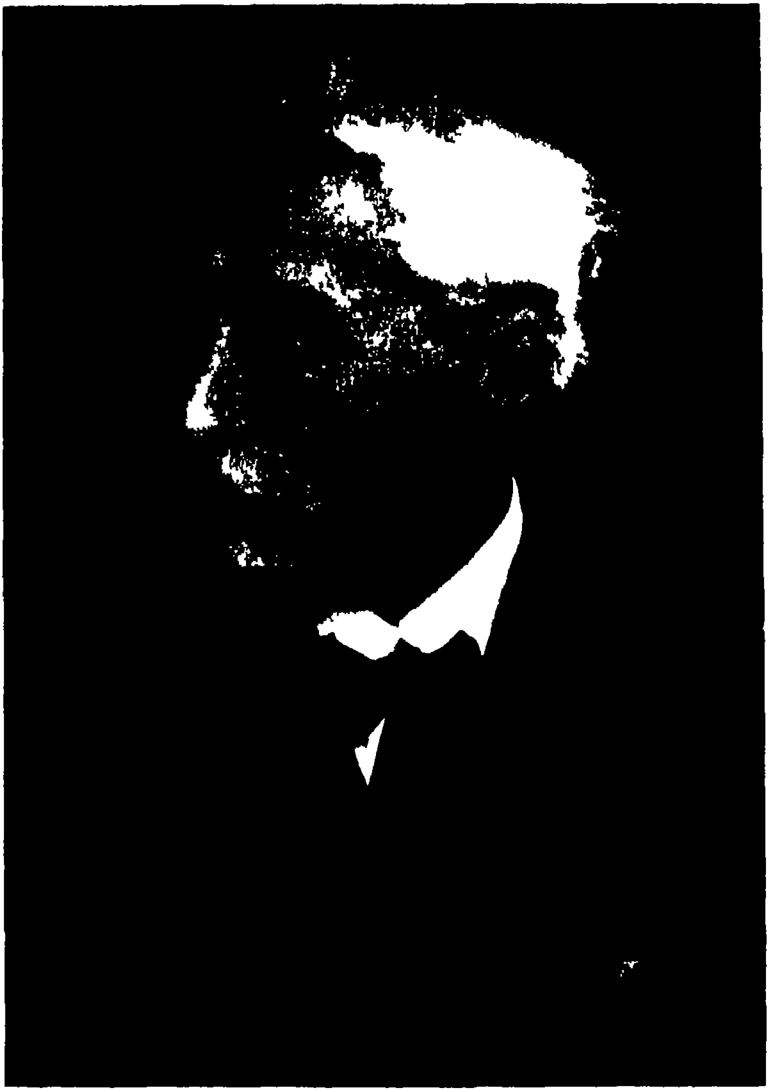
Life Composition Fund

	£ s d		£ s d
To Balance forward	35 0 0	By Balance at October 19th 1921	35 0 0

Balance Sheet

LIABILITIES		£ s d	ASSETS	£ s d
To Reserve Fund		100 0 0	By 100 6% War Loan (B E)	95 0 0
Life Composition Fund		35 0 0	30 5% (P O)	28 6 9
Revenue Account			20 5% War Bonds 1922	20 0 0
Surplus at November 21st, 1919	85 8		Newcastle Savings Bank	68 3 0
Add Surplus from November 21st, 1919 to October 19th 1921	34 1 9			
	119 9 9			
Less Transfer to Reserve Fund	43 0 0			
		76 9 9		
		£211 9 9		£211 9 9

October 20th, 1921 Audited and certified correct R. N. WILKINSON *Hon. Auditor*



Dr Merz in 1921 (*æt* 81)

University of Durham Philosophical Society

PERSONAL REMINISCENCES AND IMPRESSIONS OF DR JOHN THEODORE MERZ

I —By R A Sampson F R S

My friendship with Dr Merz dates from 1893 I had just joined the Staff of the Durham College of Science as it was then called and he lost no time in making a newcomer feel at home I recall a characteristic touch in his first note—a sketch map to pilot the stranger without waste of time to The Quarries where I afterwards spent so many an hour under his friendly and hospitable roof For he loved to talk his ideas into shape Not indeed that they ever appeared to pass through an inchoate stage Rather it seemed that when he had mastered his authorities and ranged his facts in significant historical order to his own satisfaction within his own mind he would unfold the result His first great book was at that time on the stocks It had been on the stocks for many years For how many years before that he had planned to write a *History of European Thought in the Nineteenth Century* I do not know Viewing its ambitious scope not a few who knew the multifarious calls upon his time and the meticulous care with which he discharged every duty doubted if it could ever reach the printer's hands They underrated his power of detachment his strength of purpose and above all his interest in it This interest was not so much a master-passion as a steady compelling force To Merz who was interested also in practical affairs and very successful in that sphere and highly appreciated by those who are the only competent judges erudition was a thing worth while It was *the* thing worth while This is so unusual a view that it stands out for me as a strikingly foreign trait a heritage of his German parentage ‘ The English public school teacher has a contempt for learning ’’ he said to me on one occasion. Now he never aimed at epigram, or pared a phrase from its simple truth in order to give it force. He valued rightly, and highly too, the education which our public schools supply But there was the sorry fact. He, having many calls upon his time, used to "lengthen his days," not by "stealing a few hours from the night," but by rising every morning at five, in order to devote the first and best portion of each day to the important business of reading books.

Merz's love of books showed itself in an unusual, individual way. It is related of Darwin that he had no respect for a book, as such. He would tear one in two, if in that form its contents were handier. Most of us, without going so far, keep our books in their original working jackets, or if a series, or foreign custom, compel us to bind, the book's appearance is rather less interesting after the ceremony than before. I well remember the astonishment with which I found upon Merz's shelves, clothed in faultless leather, mathematical textbooks that I had known in every stage of honourable and dishonourable decay. A copy of Cantor's *Geschichte der Mathematik* which he gave me when breaking up his mathematical collection, is one of the most perfect pieces of binding, by Zaehnsdorf, that I have ever seen. In these shapes his books proliferated from his study into every bedroom and alcove of the house—biographies, systems, histories, series of collected works. Tidiness and good workmanship were a pleasure to him, and he paid that tribute by binding the books in which the deepest part of his interests found sustenance. He said he was a slow reader, but I believe every one of his books had been read. Marginal pencil marks told the story, and as far as I could trace, their contents were always at his command, in memory, at once and without effort. The visitor to his study will recall the words:—

"Nulli optabilis dabitur mora;
Irrevocabilis labitur hora.
Ne sit inutilis semper labora,
Neve sis futilis, vigila, ora."

or roughly Englished:—

The hour is here—is gone;
Sans pause the stream flows on.
Work, that to-day bring gain,
Watch, pray, lest work be vain.

These words which met one, carved on the overmantel, had more than a casual significance. His life was directed with vigilant industry to a clearly conceived goal. I have said that his innate esteem for learning was a German trait. Many of his traits seem to me to bear the same stamp, belonging to that Germany which for the time being we can recognize no more. One of these was the dominance of the idea, the theoretic object, *der Begriff*, in his most considered actions With us, an action grows into shape as it matures. I suppose we can imagine a pure-bred Englishmen writing a History of Philosophic Thought, but the result would hardly have been so impersonal as Merz's work. It is, perhaps, a weakness that makes one ask, what he himself thought was the outcome from so many lives and labours devoted to science and philosophy. One reads an elaborate chapter on one point of view of Nature, and, approaching the end, one wishes for some integrating comment. Vain wish. One turns the page, and a new chapter, from a new point of view, begins, related as carefully, without fatigue or bias, to be replaced, when concluded, without epilogue by a third. Because the idea was to write a history, not a philosophy. Is it merely a national bias that makes one impatient that so many opportunities of making himself interesting should be passed by? We distrust the value of system and trust the original perception of individual minds, in spite of its incoherence and even of its errors.

Grau, theurer Freund, ist alle Theorie,
Und grün des Lebens gold'ner Baum.

Therefore some will esteem more highly the contents of his two later books, short but not less careful, because of their personal character, the deliberate contribution of a participant in life, and not merely the detached survey of an historian.

Merz's detachment, his limitation of interests, was unquestionably a deliberate restriction, in order that by singleness of purpose he might achieve a definite effect. In the unbounded regions in which his thoughts moved, even greater forces can become meaningless and be totally lost by diffusion. His mind was anything but diffuse. He could recall the weather of a given year and even on specific dates. I happen to remember the names of the "three bad saints"—Servatius, Bonifacius and Pancratius—who, he instructed me, so often blight the vineyards of France with frosts on their name-days in the early half of May. Many another such curious trifle, floating up from the pit of memory, bore witness to the way in which every fact that came by found lodgment, to be used in its proper place, if it had a use.

Merz used to talk very freely about business affairs and business people. He did not regard business, as so many born scholars do, in the light of a tiresome necessity, to be set aside as soon as it had served its purpose. His interest in it was active and unfeigned. But I allude to it here only so far is it made part of his private, intimate daily life. Another aspect of this life which for other reasons one does not expatiate upon, but which one cannot leave out without wholly misdescribing him, was its profoundly religious character. Indeed, no other type of mind could have written his last book. He was a very just man. And when in old age his excellent constitution began to give way, and in particular he was progressively deprived of sight, he proved a very patient one. His interest was undimmed and his work continued. Many of us will always cherish the memory of his figure, wrapped in its shawls, by his study fire or in the garden he so loved and knew in such complete detail, bringing out from the unclouded stores and pictures of his memory things new and old.

II.—By Emeritus-Professor P. Phillips Bedson, M.A., D.Sc.

No memorial of Dr. Merz would be complete without an account of the part he took in establishing Armstrong College, and although I am fully conscious of my inability to do full justice to this special phase of Dr. Merz's work, still my sincere and deep regard for his memory impels me to record my reminiscences of his activity, as a Member of Council, extending over nearly forty years.

The College Council was fortunate in counting amongst its members so distinguished a scholar as Dr. Merz, whose wide educational experience and broad sympathies were well calculated to help forward the objects of the College. Moreover, his recognized position as a writer and exponent of philosophy, gave an added distinction to the Council. When Dr. Merz was elected to the Council, the College was, although in its twelfth year, still in occupation of temporary premises. Shortly after his appointment, the question of finding more suitable quarters for the College became urgent, and afforded him an opportunity for the display of his practical knowledge of finance and affairs. In fact, his eminent gifts as organizer and man of business were destined to be of the greatest service to the Council. Dr. Merz's immediate contribution to the solution of the problem of new buildings on a new site was an exhaustive analysis of the financial position of the College. This statement was no ordinary balance-sheet, perfunctorily explained and concluding with a deficit—in University finance a never-failing feature of such documents—but a lucid, thorough and scientific analysis of the annual expenditure. One distinctive feature of his analysis was the demonstration of the fact that the College was paying extravagantly for its housing in premises entirely inadequate, and notoriously unsuitable for the purpose, and as for expansion under these conditions—impossible. Thus, the Council were heartened to proceed in real earnest with the project of securing a site upon which to erect buildings, specially planned for the needs of the College.

From this time on for many years Dr. Merz's hand guided and directed the finances of the College. His counsel was in constant request, and to his opinion the Treasurer invariably deferred. In fact, Dr. Merz became Chancellor of the Exchequer and, joining with Messrs. W. Cochrane, R. R. Redmayne and Dr. Spence Watson, spared neither time nor energy in promoting the advancement of the College. To these four men, in a special degree, do Newcastle and district owe a debt of gratitude for the growth and success of the College.

When the site had been secured and building operations begun, unremitting was the attention which the affairs of the College received from Dr. Merz, by whose instrumentality a loan was secured, making the extension of the buildings possible. During the first years in the new premises, when lack of funds threatened the very existence of the College, Dr. Merz by personal effort secured subscriptions to a sustentation fund, which helped "to carry on," until the Government with Treasury grants came to the assistance of the University Colleges in England.

Looking back to those days, when not only was money scarce, as it always has been, but when the work and the mission of the College were so little appreciated, one realizes now how the firm belief in its future by such men as Dr. Merz served to give courage and confidence to the members of the Staff, and stimulate them in their endeavours to extend the influence of the College in every possible way.

To recount in full the activity of Dr. Merz as a College Councillor would be to recite the history of innumerable committees upon which he served. No committee charged with recommendations for the appointment of a member of the Staff was deemed complete without him, and to all this work he willingly gave of his best.

To one whose sympathies were those of the scholar and student, education in Science, pure and applied, alone did not constitute a sufficient justification for the existence of a college in Newcastle. Dr. Merz showed his practical sympathy in every endeavour to widen and broaden the curriculum, and thus provide a centre for the higher general education of all properly qualified to benefit from it.

In the College Library Dr. Merz took a special interest and to it he presented the important collection of scientific and philosophical works which served him for reference and study in the preparation of his great work, *The History of European Thought in the Nineteenth Century.* Upon the possession of the Merz Library the College Authorities are to be congratulated, and upon them now devolves the care and solicitude which Dr. Merz was wont to bestow upon his books.

These reminiscences would not be complete without reference to the fact that for the greater portion of his life Dr. Merz was engaged in Chemical Industry. To many he was known chiefly as a chemical manufacturer. At one time he was engaged in the Tharsis Sulphur and Copper Works, both in Glasgow and on Tyneside; and he also attempted to establish a factory for the production of Coal Tar Colours at Hebburn-on-Tyne. He had studied Chemistry at the University of Gottingen, where he had been a pupil of Wohler, Professor of Chemistry in that University, who is famed as the discoverer of the artificial production of Urea. This, the first instance of the preparation in the laboratory of an animal product, laid the foundation of Synthetic Organic Chemistry. Wohler himself had been a pupil of Berzelius, and for many years collaborated with Liebig in work of fundamental importance to Chemistry. Thus Dr. Merz afforded a direct link with the great founders of Chemical Science. For the student of Chemistry, the first volume of the *History of European Thought in the Nineteenth Century* contains a masterly account of the development of chemical theories.

Although Dr. Merz did not appear to participate much in the public affairs of his adopted city, still, by his quiet unostentatious work he not only helped to build up Armstrong College to be a centre of light for future generations, but to his inspiration Newcastle and the surrounding District are indebted for the establishment of an Electric Supply, recognised throughout this Country as the pioneer and type of all such enterprizes

This tribute to the memory of Dr Merz would be incomplete without a reference to that sympathetic friendliness and interest, which characterised his attitude towards the members of the College Staff, many of whom, like myself, will ever gratefully cherish the memory of the friendship we had the great privilege to enjoy

III.—By Professor DOUGLAS A. GILCHRIST, M.Sc.

I came in contact with Dr. Merz at the end of 1902. Informed that he was one of the "Big Four" in the Durham College of Science (now Armstrong College), I soon realized how valuable the all-round attainments of Dr. Merz had proved in its early years, possessing as he did that rare combination of business aptitude, power of organisation, and keenness for the development of complete Higher Education in the Humanities, in the Sciences, and in the various branches of Industrial Activity. He believed that the "together view" was the bed rock of higher education, and realised that the specialist, whether in Greek or in Agriculture, must not only have a broad training and outlook, but that his general culture must be developed along with his special knowledge.

The impression given by those parts of Dr. Merz's works which I have read is that he had little sympathy for philosophers who did not possess a wide range of knowledge, *e.g.*, he pointed out that Bacon did not recognise the importance of mathematics. He evidently placed much value on the close connection between philosophy and general literature in this country—a feature not nearly so prominent in France and Germany—and, as a consequence of this, he held that philosophy had a close alliance with the practical problems, as well as the literary tastes, of Britain. My feeling is that Dr. Merz's life and work should do much to stimulate the fuller appreciation of this relationship by our students of philosophy. Even as it is, it seems to me that the influence which British literature has had on British philosophy has done much to make the thought of our philosophers more democratic in character than that of France and Germany, and that philosophy in Britain has been distinguished by a healthy common-sense, because it has not been confined to a few specialists discussing highly abstract problems in highly technical language. When Dr. Merz suggested that the dictum, "I think, therefore I am," would be truer if rendered, "thinking is being," I suggest that possibly thoughts like the foregoing may have been in his mind. At any rate, this rendering of his was in keeping with his complete unselfishness, due undoubtedly to his aspirations to truth and love.

> "If self the wavering balance shake
> It's rarely right-adjusted."

and—

> "O! wad some po'er the giftie gie us
> To see o'orsels as ithers see us!"

It is in the spirit of these quotations that Dr. Merz seems to me to have reasoned in arriving at the conclusions expressed in his *Fragment on the Human Mind*, in which intense philosophical thought is put in a form which enables even a lay-reader to follow the author a considerable way in his penetrating arguments.

Some years ago he commented to me on the truth and beauty of Blackmore's translation of the *Georgics*. Our examination of it showed how much Blackmore has done to assist the reader to separate Virgil's record of the folklore and of the actual facts of his time.

A talk with Dr. Merz on Goethe, or Burns, or any great British, French, or German poet, was a rare treat, as he could so aptly give quotations to illustrate their different phases of thought and to show how their loves, their ambitions, and their sorrows had influenced their work. How he would tell of the influence of *The Vicar of Wakefield* on young Goethe and what a stir the appearance of Macpherson's *Ossian* created throughout Europe! When the talk was in his library and an author was mentioned, how readily he could refer to his work and the best information about him! His library was that of a man who had his favourite authors at his elbow, in print and bindings worthy of them, and surrounded by other books so chosen as to constitute an unique reference to British, French and German literature. His gift of his scientific and philosophical books to Armstrong College was a great acquisition to the College Library.

In his *Fragment*, the chapter "Of Truth" gives a rare insight into the man himself. He tells us there that Truth leads us to the Beautiful, the Good and the Holy. His belief was that the full Truth we should strive after comprises Beauty in Life and Love, in Music, Poetry and Art, and in Industry, and that our humbly striving after ultimate truth will develop beauty, love and holiness in our lives.

It was my great privilege to be his near neighbour and in close touch with him during the last two years of his life. I was greatly impressed with his power in conversation of imparting correct and concise knowledge of its subject, and how he would frequently divert it so that enlightenment on another subject might be obtained from someone present who had special knowledge of it.

How he loved his home and garden! Every plant in the latter recalled rich associations to him. In no way was his comprehensive outlook better illustrated than by the unique knowledge he had of the science and the art of the garden, which were, after all, two of the portals to his keen enjoyment of it. In his garden

"................ he outwent
A royal fortune in his heart's content."

His decision, when young, to widen his horizon from the philosophy of the Schools to the philosophy of the World has borne rich fruit in many directions, as in the encouragement he gave to the academic and business circles of Newcastle to draw more closely together. This was a practical application of his "synoptic," or "together," view. The ideal of synopsis probably gives the best indication of his method of work and study. One of his favourite illustrations of this view was that a child's conception of an egg already contains implicitly all the aspects which the sciences afterwards isolate and abstract. The egg may be analysed by a chemist into its chemical constituents, and studied by a biologist in respect of its structure and development into a living animal, but if they lose their primary conception of it in its *wholeness*, their ultimate conception will be impoverished by abstraction and confused by the details of their analytical work.

My belief is that, to those who knew him, the character of the man is revealed to much the greatest extent in *Religion and Science.* In this book he faces the problem of the means leading to a scientific conviction. "It is frequently the flash of the moment which throws light upon the whole region of painfully acquired information and gives it life, a stimulating and impelling force. Hundreds of well-informed persons lack the finishing touch which gives life to their knowledge, and much so-called education seems quite incapable of imparting it." This flash of synoptic insight not only yields our convictions in Science and, by a different process, in Religion, but it should also be what all teaching and research work should aim at producing.

The foregoing quotation summarizes the ideas given to me by many talks with Dr. Merz as to how research and educational work should be conducted. In Agriculture, for instance, results of such work should not be announced to the farmer till they have been thoroughly tested under conditions of farm practice and shown to be more economical than his own probably sound methods. All teaching should be suggestive rather than dogmatic and should encourage the farmer, the student, and the research worker to look for development, so that a living interest as well as continuity of progress is secured.

I know what a keen interest Dr. Merz took in the Agricultural Department of Armstrong College when Dr. Somerville, who so well laid its foundations, was establishing a degree course and shorter courses in Agriculture. conducting field- and other experiments throughout the four Northern Counties, and starting so efficiently the Northumberland Agricultural Experiments Station at Cockle Park, the results from which are now known throughout the world.

Dr. Merz, as Chairman of the Board of Directors of the Blaydon Manure Company, always welcomed visits of the College Students and Staff to the Works of his Company, and arranged demonstrations of a most valuable character, every possible information being given as to the manufacture of the manures. He took the keenest interest in the research and the teaching work of the Department and was ready with suggestions. He frequently drew my attention to recent work in Europe and America, and some books he obtained from a friend in the United States have been of great service in trials we are making of ground mineral phosphates, which promise most valuable results. Shortly before his death, he was trying to ascertain to what extent agricultural education and research had been productive of results, and how these could become more effective. We both recognized the great advances that had been made in the breeding and feeding of live stock, and what the plant breeder and the research worker in manures and soil cultivation had done for crops, but were not so able to decide to what extent modern social conditions had checked this greater production. If the farmer and farm labourer had continued working long hours and living as frugally as in the past, the modern equipment would have greatly increased the agricultural output, but, as in other industries, the facilities for increased output have been largely utilized to provide more comfortable living and easier and better conditions of labour.

Some years ago he accompanied me round Cockle Park with a party of farmers. He was most appreciative of the experiments, the obvious results of which pointed to improvements in farm practice, and increased economic output of crops and meat, and was especially pleased when he found the farmers keenly interested and expressing determination to put the results into practice at home.

IV.—By Professor Alexander Meek, D.Sc.

Dr. Merz was known to all of us as a man sincerely interested in the two Colleges at Newcastle and in our University, and as a keen and active member of their Councils and of the Senate. When I first met him, now over 25 years ago, I was studying several features of the growth of organs in relation to the growth of the animal, and it was a great pleasure to me to find in him a philosopher impressed with the Darwinian doctrine of the survival of the fittest and eager to discuss evidences of the competition of parts within the organism: above all, in possession of the literature dealing with the work of Roux, Mehnert, and others, on Entwickelungs-and Bio-mechanik.

He was then busy with the *History of European Thought*, the stupendous and encyclopædic task of critically analysing the progress of science and philosophy [illegible] the 19th century. In his masterly survey we can trace the independent and yet interdependent growth of Science in its various branches. Each was emerging at the beginning of the period from a condition of relative chaos. During the century discoveries were made on all sides, and generalizations of great importance arrived at, but, amongst the many achievements, one stands out as producing in England, and, later, on the continent, a change in outlook which affected all phases of thought, viz., the discovery of the fact of evolution. The acceptance of this fact—and the acceptance took place rapidly—added the dimension of time to the conception of space, introduced an appeal to history and change in solving problems relating to the universe and life, and it offered a rallying ground for harmonizing the results already achieved in the realm of science, and even in that of philosophical thought.

The work of the geologist had reached a stage which permitted Lyell and others to teach that the earth has had an origin and a continuous history. The biologist was ready to receive the doctrine of the evolution of life, with the implication that life on the earth had a beginning, and

Darwin, after originating the new view, owed much to his friend Huxley, and also to Wallace who had arrived independently at the same theory, for its spread. The light thus shed on the facts of palæontology, brought into line with those relating to the genealogies of plants and animals, made clear the nature of the problems of morphology, physiology and embryology. It even bent its beams on psychology, for it became evident that the progress of life from stage to stage was not merely a morphological and physiological gain, but was accompanied by a corresponding gain of mental power. The student of life has now the knowledge that practically all phases of life exist at the present day, that the conspicuous evolutionary changes only occurred once and were never repeated, that all start at the beginning and encompass, each of them, a morphological, physiological and psychological history to the stage they occupy. He is conscious also of the long history of life and recognizes that on all planes of life progress has been made in brains as well as in structure and function, except where living forms have yielded to the allurements of undue protection.

The culminating result of the evolution of animal life is man, and those reared in the materialistic schools have, at all events to begin with, to view man as part of the whole. In one respect man is as old as life on earth. In the other, as man, his reign is a short one compared with that of the life from which he emerged. During the eras of millions of years prior to man, there was no science, no philosophy, no religion. These have, like man, an origin and an evolution.

This may be said to have been the condition of scientific thought in Britain and America before the end of the 19th century, and the doctrine of evolution may now be said to be generally accepted wherever scientific work is prosecuted and taught.

Such a presentation of the origin and history of man, man viewed in relation to the background of the history of life, has penetrated slowly into philosophical thought. Much good work was done towards the end of the last century—for example, by Lloyd Morgan; but it is since the beginning of the 20th century that animal life has been studied with the express view of gaining more definite information concerning the mental processes of animals. Dr. Merz, after he completed his *History*, was keenly interested in the work of the "behaviourists" and in all observations on the habits of animals which tended to illustrate their mental capabilities. As recently as 1921 he requested me to tell him more particularly the details of an observation I had made and published in 1901 (Report of the Cullercoats Laboratory for that year, p. 59). I am constrained to recount the incident here, for it illustrates, with many another, that even lowly animals may evince mental qualities of a noteworthy kind.

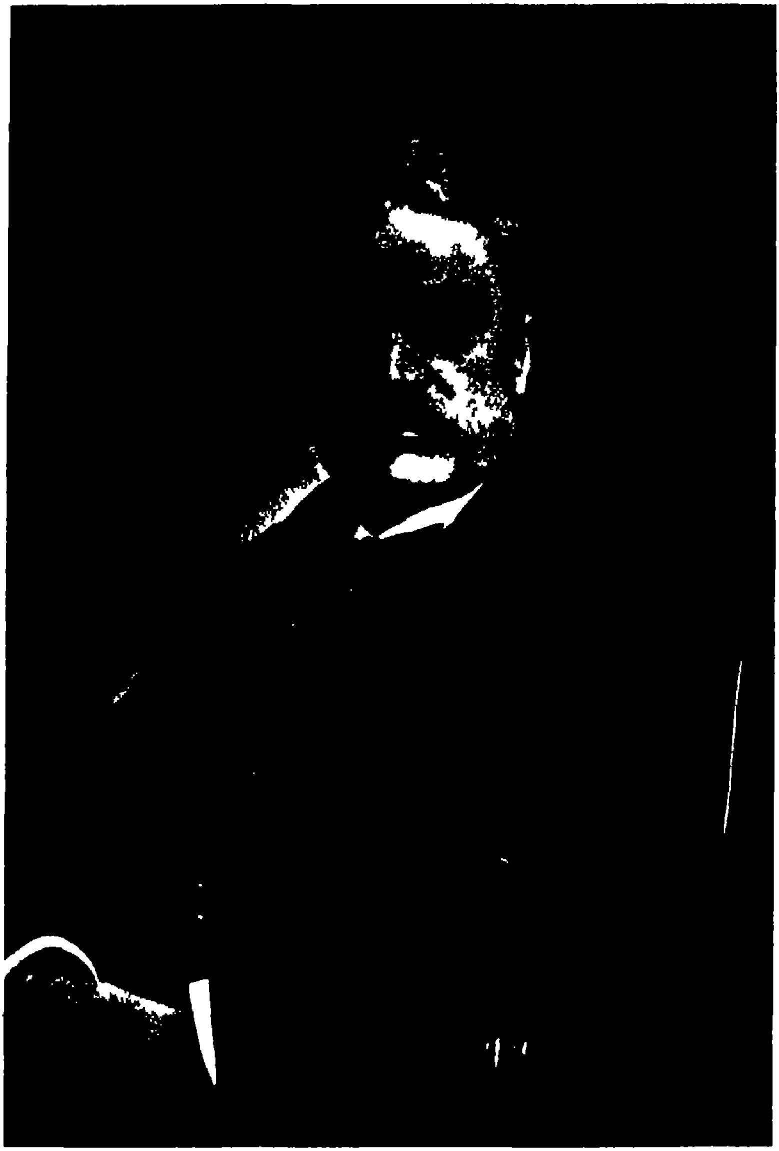
The creature in question was a small amphipod crustacean with the name *Siphonoecetes colletti*. Its brain is quite microscopic. It lives in sand just beyond tide marks, and constructs of sand grains a tube open at the ends, and just of the right size to afford complete protection. I took one specimen out of his tube, placed him in a Petri dish filled with sand and containing another specimen in his tube, and watched the newcomer's movements under the microscope. After some exploration he came across the tube and, finding it occupied, determined to evict the tenant and gain it himself This is how he accomplished his object. He first made a display with his antennæ at the front entrance, next at the back entrance, evidently with a view, not so much at intimidation, as to convey to the occupant that he was merely making sure the tube was already occupied. He actually repeated the process several times in rapid succession after which he lay quietly alongside the tube. The occupant also remained quiet for a while, then began to make exploratory protrusions with his antennæ; at first to a very short distance, then gaining confidence the antennæ were produced to a greater distance. The raider meantime never gave the least sign of movement, waiting patiently and watching the movements of the tenant until the moment came for action. He knew as well as I did that he could not gain entrance until the occupant concluded that it was safe to emerge far enough to allow the eyes, that is to say, the head, to clear the tube. Something at the right time conveyed to the raider that the occupant in the process of emerging was going to do this. Thus, as the occupant was emerging, he gently retreated, and just as the other's eyes were clearing the front end of the tube, the raider plunged suddenly and accurately into the hinder end of the tube and thrust out the occupant in front of him. The latter turned round to look at the tube and then began to survey the area, soon settling down to the task of making a new tube, in itself a most interesting proceeding. Such an incident cannot be explained by reference to tactic responses The late occupant did not make the least attempt to gain an entrance after the eviction. Moreover the proceeding does not always succeed. I next placed two examples in a small dish with only one tube. One of them almost immediately entered into habitation. The other tried his best to evict him, but used clumsy, forceful methods, in his efforts dismantling part of the tube, and he failed in his object.

The results obtained by experimental psychology, taken together with such observations as that illustrated above, give reason to believe that mind has its history and evolution. When the conclusions which behaviourists reach on the basis of their laboratory experiments are referred for confirmation to the actual distribution of creatures in their given environment, it is seen that behaviouristic principles cannot adequately explain that distribution, and that the factor of volition is throughout at work.

A farmer and his dog looking at a passing train are equally aware of the fact. So are all the birds and beasts in the field near the railway. The wave communicated through the rails to the soil is conveyed through the earth to the subterranean mole and to the rabbit in its burrow. It is communicated to the multitudes of earthworms underfoot, and is dispersed in the plants, even reaching the latest formed branches of the trees, and in the rattle of the window back there in the house (which conveys to master and dog alike, when in the house, the fact of the train). The wave is conveyed to the pond, and every living creature in it is aware of it, and some probably recognize its periodicity. The passing of the train affords us every phase of effect, from the moving of the particles of the soil, and the reflex of Vorticella, to the mental processes in the dog and in man; from the reception of the wave by inanimate objects to the manifestation of the reception indicated by many of the animate recipients.

This is how the problem unfolds itself to the zoologist, the neurologist and the physiologist. While these admit that philosophy is the greatest of all sciences, as seeking to unify the truths proclaimed by each, and even to discover ultimate truths, they feel that progress now does not lie in introspection and debating the claims of schools of thought, but in beginning, anew and humbly, an enquiry into psychological manifestations in life generally, with the tentative view at least that the human brain is the product of evolution.

I know from my conversations with Dr. Merz that he was sympathetic with this view. He fully realized that it was impossible to make progress in biology with purely mechanistic or even vitalistic concepts, and he insisted on the application of the genetic point of view to mind. He saw that neither purely physiological methods nor metaphysical ones lead anywhere in biology. The conviction which some of us hold, that on many planes of life consciousness may be said to have emerged in varying forms and degrees, and that its presence may be demonstrated where the conditions call for its manifestation—this conviction is wholly in harmony with his views.



Dr. Merz about 1900 (*æt.* 60.)

THE PHILOSOPHICAL WORK OF DR. JOHN THEODORE MERZ.

By Professor R. F. ALFRED HOERNLÉ, M.A., B.Sc.

Mr. Bertrand Russell, in one of his epigrammatic moods, has recently observed that " philosophers, though they are all agreed that philosophy is important, are agreed about very little else " One might spin out this remark by adding that philosophers, even though they are agreed upon the importance of philosophy, generally disagree concerning the reasons why it is important. Nay, it is even possible to find philosophers—Mr. Russell himself is an example—who disagree with themselves on this point, praising philosophy at different times on different, and sometimes mutually incompatible, grounds.

However this may be, there is no doubt that Dr. Merz thought philosophy supremely important and that, throughout his life, he never wavered in his reasons for thinking so. Moreover, he is as clear about the great task which, in his view, belongs to philosophy, and to philosophy alone, as he is clear about what he considers the best lines along which philosophy may hope to fulfil that task. A review of his philosophical life-work, such as is here to be attempted, will, therefore, appropriately deal with two topics. (1) The first is the reconciliation of science and religion, the discord of which threatens to destroy the unity of our spiritual life. To effect this reconciliation is, for Dr. Merz, the distinctive function of philosophy. To the fulfilment of this task he bent his own best energies, and our first duty, therefore, is to review his contributions to the discussion of this central issue. (2) And, secondly, we must consider the " synoptic " method by which Dr. Merz seeks to harmonize the world-views of religion and of science, and which he also applies, incidentally, to the solution of a number of technical problems, such as the different senses of " reality " ; the distinctions between the " inner " and the " outer " world, and between " subject " and " object "; the nature of " primordial experience"; the relation of body and mind; etc. Dr. Merz's discussion of these will furnish our second topic.

Throughout, our review will make no pretence of being exhaustive. Its aim is solely to focus attention upon the points which were most central in Dr. Merz's thought and also most distinctive and, in part, original. Of the (at least relative) novelty and originality of some of these points Dr. Merz was himself profoundly convinced. The deep and comprehensive studies in the history of modern European thought which occupied most of his life, led him to the conclusion that many contemporary thinkers had, under the influence of natural science, on the one side, and of Hegel's dialectical method, on the other, turned aside from another way of philosophizing the inception of which he traced back to Hume and Locke and Bacon, and to the elaboration of which he thought that the future belonged. So convinced was he of this that during the last months of his life he began a work, unfortunately cut short by death, on the theses of the "New" Philosophy. It is a remarkable fact that Dr. Merz, whose own thought was so profoundly influenced by Schleiermacher and Lotze, and whose knowledge of German philosophy was so extensive that some critics even accused him of having given undue space to German thinkers in his *History of European Thought in the XIXth Century*, should have found in the method of the British school, reinforced by the resources of modern introspective psychology, the most hopeful method for the philosophy of the future. That in the evening of his life his own strength was unequal to the task of building up the "new" philosophy in detail, he was fully aware. But the splendid opportunity which seemed to him here to lie ready to the hand of a younger man, was a favourite topic of conversation with him. On such occasions he would speak like a philosophical Moses, seeing from afar the promised land which he knew himself not to be destined to enter.

But, before turning to the proper topics of this paper, it will be well to give a brief account of Dr. Merz's philosophical publications in their historical order.

I.

The earliest work by Dr. Merz on a philosophical subject* was, probably, an article in Gelzer's *Protestantische Monatsblatter* (1864) on *Francis Bacon von Verulam: Seine Stellung in der Kulturgeschichte.*† From private papers in the possession of the Merz-family it appears that this article was originally prepared for a small Philosophical Society at Heidelberg, whither Dr. Merz had gone in 1862 for post-graduate studies in philosophy. The Society was composed of the Professors and Lecturers in Philosophy and of some of the senior students, the leading figure being Eduard Zeller, the well-known author of the *History of Greek Philosophy.* The occasion for Dr. Merz's paper was the appearance of a pamphlet by the famous chemist, Justus von Liebig, entitled *Francis Bacon und die Methode der Naturwissenschaft.* Young Merz set himself to defend Bacon's analysis of scientific method against Liebig's criticisms. The discussion which followed revealed a marked difference of view between Zeller and Merz, which was intensified when the latter, in accordance with academic custom, presented to the Philosophical Faculty a thesis on *The Present State and Task of Philosophy* for the *venia legendi.* Under Zeller's influence, the Faculty, whilst acknowledging the literary ability, general knowledge, and independent attitude of the writer, refused to accept the thesis and asked, instead, for a more technical *specimen eruditionis.* This experience determined Dr. Merz to leave Heidelberg and seek his academic fortune in Bonn. There, not only was his thesis accepted, but he also obtained the *venia legendi* after a *colloquium* with the Faculty on a further thesis which dealt with *The Relation of Logic to the Theory of Knowledge.* This latter thesis, which does not appear to have been published, once more formed the basis of the *colloquium* with the Philosophical Faculty at Göttingen, when, after a year at Bonn, Dr. Merz joined the circle of lecturers on, and students of, philosophy which had gathered at Gottingen round Herman Lotze. Dr. Merz's inaugural lecture at Bonn dealt with the importance of Kant's philosophy for contemporary thought, and appeared, also in 1864, in Gelzer's *Monatsblätter* under the title *Über die Bedeutung der Kantischen Philosophie fur die Gegenwart* (Vol. 24, pp. 375-389). About the same time, the original Heidelberg thesis, or portions of it, appeared in the *Deutsche Vierteljahrsschrift* (Vol. xxvii., No. 106, April-June, 1864; pp. 38-76) under the title, *Zur Verständigung uber die kulturgeschichtliche Stellung und Bedeutung der deutschen Philosophie im neunzehnten Jahrhundert.* This youthful work is interesting because of the unmistakeable anticipations which it contains of some of the positions which, towards the end of his life, Dr. Merz developed into the programme of the " new " philosophy. Writing at a time when the reaction against Hegelian idealism in Germany was at its height, and when the decline and fall of all philosophy before the rising sun of positive science was confidently predicted, Dr. Merz enters the lists as an undaunted champion of philosophy. He agrees, indeed, that the philosophical constructions of the preceding age cannot stand. Kant's dualism is unsatisfactory; Fichte fails to provide an adequate basis for religion; Schopenhauer's cosmic will is a daring but unsound hypothesis; Hegal's philosophy a " *konstruierender Schematismus.*" But these, and other failures, leave the need for philosophy untouched. The abiding task of philosophy continues to confront it, and new sources of knowledge—the rising sciences of anthropology and ethnology, of the physiology of the senses, and, above all, of psychology, together with new researches into the history of philosophy itself—are providing fresh data for a fresh attempt at a philosophical synthesis. Moreover, such a synthesis must include, not only scientific and philosophical theories, but also contemporary movements in art, literature, morals and religion. It must be, in fact, a synopsis—critical in method, but positive in result—of human *Geistesleben.* It must be " *der alle Kreise des geistigen Lebens einschliessende, nicht aus-*

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* His Doctor's Thesis, in 1862, was still connected with the mathematical and physical studies which had occupied him as an undergraduate: *Über die Rückkehr eines Planeten an den nämlichen geocentrischen Punkt des Himmels.*

† *l.c.* vol. xxiv., pp. 166-196.

schliessende, Kreis." In this emphasis on synopis (though this striking term is not yet used); on the reconciliation of conflicts in the spiritual life of humanity; on the inclusion of literature, art, and morals; and, especially, on the central importance of psychology, it is easy to discern the seed of which we have the flower and fruit in the matured conclusions of Dr. Merz's later works.[*]

It is interesting, in this connection, to record the subjects of the lecture courses which Dr. Merz gave as *Privat-Docent*. At Bonn, he lectured, publicly, on "The Position of German Philosophy during the XIXth Century," and, privately, on "The Introduction to Philosophy and Philosophical Encyclopædia." At Gottingen, in 1865-6, he gave a private course on "The History of German Philosophy since Kant," and a public course on what was to remain the central problem of his philosophical thinking, viz., "The Relation of Philosophy to Religion."

It was in 1865, too, that he read, for the first time, through the whole of Hume's works, and began to project a work to be entitled, "David Hume, his Philosophy, his Age, his Influence." From this time, therefore, dates the marked influence of Hume's thought on Dr. Merz. The book on Hume, however, remained unwritten, for towards the end of 1866 Dr. Merz decided to abandon the academic career, in spite of the fact that his work had begun to attract attention and that he was being mentioned for a Professorship at the University of Basel.[†] The reasons why Dr. Merz gave up a career in which he had every prospect of achieving distinction, were, partly, that an illness in 1866 had left, as an after-result, a weakness of the throat which would have proved a serious handicap in lecturing to large classes, and, secondly, that he felt himself to be too young and immature for writing a *magnum opus*, such as was expected from a candidate for a professorial chair. Hence, he decided to avail himself of an opportunity which presented itself to him at that time, for returning to the land of his birth, and to devote his high scientific training to the development of the chemical and electrical industries which were then being inaugurated in this country. However, in the midst of his activities as industrial organizer and administrator, he kept alive his interest in, and love for, philosophy. When, in 1877, Edward Caird's first book on Kant, *A Critical Account of the Philosophy of Kant*, appeared, Dr. Merz contributed a review-article on it to *Macmillan's Magazine* (Vol. xxxviii, May, 1878, pp. 67-80). The scholarly competence of this article drew attention to his qualifications as an expert on German philosophy and led to his being asked to contribute the article on *Lotze* to the Ninth Edition of the *Encyclopædia Britanica* (1883). With small additions (mainly bibliographical) this article has kept its place in all subsequent editions of the *Encyclopædia*.* An even more significant recognition of Dr. Merz's philosophical scholarship was his inclusion among the select group of contributors to Blackwood's *Philosophical Classics*. His volume on *Leibniz* in that series appeared in 1884, and its merit is sufficiently attested by the fact that a German translation of it was published within two years (1886).

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[*] During these years of study and teaching at Heidelberg and Bonn, Dr. Merz also contributed reviews of philosophical books to the *Göttinger Gelehrte Anzeigen*. No less than three appear in the volume for 1864, viz., in No. 8 a review of R. Schellwien, *Sein und Bewusstsein*; in No. 30 one of Sir James Mackintosh, *A Dissertation on the Progress of Ethical Philosophy chiefly during the Seventeenth and Eighteenth Centuries* (third edition); in No. 49 one of Dean Mansel's *Metaphysics, or the Philosophy of Consciousness, Phenomenal and Real*. In 1863, whilst still a student at Heidelberg, Dr. Merz published a review of *Jens Baggesen's Philosophischer Nachlass* in the *Theologische Studien und Kritiken von Uthmann und Rothe*.

[†] The friend who urged Dr. Merz to become a candidate for the Chair of Philosophy at Basel was the well-known Classical scholar, Professor Friedrich W. Ritschl, then at Leipzig, whose friendship Dr. Merz had gained at Bonn. It is interesting to note that it was on Ritschl's recommendation that, two years later (1869), Friedrich Nietzsche was appointed to the Chair of Classical Philology at Basel.

By this time, Dr. Merz had achieved a leading position for himself in the building up and organising both of chemical and of electrical works in the North of England. Some of these, like the Newcastle Electric Supply Co., have under his guidance grown to be among the largest undertakings of their kind in this country. Yet, in spite of the fact that he was thus spending what most men would regard as the normal working hours of the day in the application of his scientific training and his organising ability to the creation of industrial works on a large scale, he conceived, early in the 'eighties, the plan of writing a *History of European Thought in the XIXth Century*. After fourteen years of preparatory reading, covering the scientific and philosophical literature of England, Germany and France, the first volume of this monumental work appeared in 1896, and was followed by the second in 1903, the third in 1912, and the last in 1914.* The fame of Dr. Merz will always rest securely on the solid worth of this *magnum opus* which is equally distinguished by the range and accuracy of its learning, the lucidity of its style, and the sanity of its judgments. It is a work the writing of which demanded unusual qualifications, such as were, perhaps, possessed by none of his contemporaries in quite the same measure. For, apart from the command of three languages, this history required an equal mastery of the thought and terminology of mathematics, of all the main natural sciences from physics and astronomy to geology, and of philosophy. Moreover, in its original conception, the history was to have had a fifth and a sixth volume dealing with the movements of thought in poetical and religious literature—thus realizing the plan of a complete synopsis of the *Geistesleben* of the XIXth century foreshadowed in Dr. Merz's youthful dissertation. We, of a lesser and lazier generation, may well be awed by the reminder that the preparatory studies for, and the composition of, the *History* were accomplished by Dr. Merz mainly in the early morning hours between 5 and 8, before his day's work as manager and director of industrial concerns began. His method was to write out a draft of each chapter in a clear, neat hand, the few corrections showing how thoroughly the material had been mastered and the argument thought out before pen was put to paper. Subsequently, the whole draft was carefully revised, and this was the occasion for the insertion of the many long footnotes, which bring the references to the relevant literature up-to-date, and display the vast stores of knowledge out of which the *History* was written. The work at once, and deservedly, took a high place in the opinion of scholars, for it appeals equally to the specialist interested in the history of his own special branch of knowledge, and to the student desiring to survey the whole stream of human thought in the XIXth century.

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* An article on Lotze for *Mind* was planned about this time, but apparently never finished for publication.

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* The first volume reached a second edition in 1903 and a third in 1907. The second volume reached a second edition in 1912.

Yet it is curious to reflect that this *magnum opus*, conceived on a scale which, even after 32 years, compelled the author to leave it unfinished, was, after all, to himself merely a preliminary " preparation " for the main aim of all his philosophical endeavours, viz., a clearer definition of his own position. Dr. Merz set himself to master the thought of a century than which none has been richer in manifold literary, scientific, and philosophical achievements, in order that from this ample soil, thus amply tilled, he might draw the harvest of his own philosophical conclusions. Or, to change the metaphor, he deliberately exposed himself to all the winds of the intellectual heaven, determined to use them all in steering the barque of his own speculation into a safe port. Among his papers, after his death, were found MSS. on *The Relation of Goethe to the Thought of the Century*, on *Goethe and Wordsworth*, on *Matthew Arnold*, and fragments of an essay on *Samuel Taylor Coleridge*—no doubt, studies preliminary to the last two volumes which remained unwritten It is well to remind ourselves of these, lest we forget that the *History*, as it stands, by no means reflects *all* the influences which helped to shape Dr Merz's final conclusions And the reminder may help us, too, to appreciate something of the tragedy—the common human tragedy of disproportion between plan and execution, high aim and human limitation—which lies in the fact that a work planned as a preliminary absorbed in the end Dr. Merz's best years and energies, and thus robbed him and us of that full development of his philosophical position to the statement of which it was to have led up. This reflection, of course, detracts in no way from the abiding value of the achievement which the *History* represents. On the contrary, it is by the

History more than by anything else which he has written that Dr Merz will live, as he deserves to live, in the minds of future generations of thinkers But, his own early and enduring hope and ambition, undoubtedly, was to bring all his historical studies to fruition in a positive philosophical synopsis Instead, *Religion and Science A Philosophical Essay* (1915) and *A Fragment on the Human Mind* (1919) contain all that he lived to develop of his own philosophical theories, and he himself was fully aware—as, indeed, the term "fragment" indicates—of the inevitable incompleteness of these statements of his views What we get in these two books, are, so to speak, sketches of the edifice to be built, together with suggestions for a method of building it and stones to be used, here and there by the builders We certainly do not get the completed edifice itself Nevertheless, any review of Dr Merz's philosophical work must be based mainly on these two books, supplemented by the *Introductions* to Vols I and III of the *History* and also by its concluding chapters (Vol IV, chs XI and XII)

We owe it, too, to Dr Merz's memory to record in this retrospect the ways in which he helped to foster interest in philosophy in Newcastle, and the contributions which he made to our own Durham University Philosophical Society Both in the autumn of 1882 and in the winter of 1894/5 he gave at the Newcastle Literary and Philosophical Society a course of *Six Introductory Lectures to the Study of Mental Philosophy*—the only occasions I believe, on which, notwithstanding the second adjective in its title, the "Lit and Phil" has had a course of lectures in philosophy, certainly the only occasion on which such a course has been given by a member of its own Committee To the Philosophical Society he read, on March 4th, 1897, a paper on *Education and Instruction in England and Abroad*, which was subsequently printed in the *Journal* of the University And after the Society had begun to publish *Proceedings* of its own, he read to it three papers, entitled *The Development of Mathematics in the XIXth Cent* (*Proc*, Vol ii, Pt 3, 1903), *On a General Tendency of Thought during the Second Half of the XIXth Cent.*

(*ibid.*, Vol. iii., Pt. 5; 1910), and *On the Synoptic Aspect of Reality* (*ibid.*, Vol. v., Pt. 1; 1913). These papers, too, have been consulted in writing the following review.

II.

We turn, then, to our first topic—Dr. Merz's conception of philosophy as the mediator between science and religion.

In the *Preface* to the *Fragment* (p. vii.) Dr. Merz explicitly declares the reconciliation of Science and Religion to be, for him, the main problem of philosophy. To the answer to this problem he had already devoted the essay entitled *Religion and Science*, and there can be no doubt that this problem was to him personally of fundamental concern. A Christian by sincere conviction, and, at the same time, a lifelong student of the theories and methods of modern science, he looked to philosophy to bridge for him, and for all others similarly brought up in two distinct traditions, the gap, and, indeed the conflict, between the two world-views. "To harmonise the essential truths of the Christian religion . . . with the unfettered progress of free inquiry"; to "effect a reconciliation between . . . the region of methodical thought on the one side, and the region of personal convictions on the other"; or, in language borrowed from Lotze, "to show how the world of values finds its realisation in the world of things" —these are some of the varied formulæ in which he sets forth the mediating function of philosophy. Again, with an echo of the language of his youthful dissertation, we read of philosophy as "the endeavour to impart unity and consistency to the scattered thoughts of general culture." The "unification of knowledge and thought" and the "formation of a reasoned creed" are to be its goal. In this conception of the task of philosophy Dr. Merz reveals his critical independence of the spirit of an age in which Agnosticism and Materialism were in fashion, and in which the theory of evolution had kindled anew the conflict between scientific theory and biblical cosmogony. And he shows himself, also, the true disciple of Lotze, of whom he says in his *History* (Vol. iv., p. 666): "We can recognise in Lotze's system the only adequate attempt to give the rationale of scientific thought on the one hand, and of religious thought on the other, and to bring the two aspects together into some intelligible scheme or formula." That Dr. Merz should have found in other contemporary thinkers from whose views he differed widely, such as Herbert Spencer and Auguste Comte, a similar demand for a reconciliation of science and religion, cannot have failed to confirm him in a view to which, anyhow, he must have been driven by personal experience of their conflict in his own life and thought, and in the lives and thoughts of educated men all around him.

In his last utterance on this subject, in the *Fragment* (p. 243), he even defines the task of philosophy as a *threefold* one. It has not only "to unify thought as displayed in the three independent regions of Science, Art, and Morality," and, secondly, "to demonstrate the possibility of Religion," but it has also, thirdly, "to afford such a view of the world and life as will support the moral structure of society." The new note which Dr. Merz here strikes, reflects, we can hardly doubt, the impression (which gained upon him during his later years) of a profound moral unsettlement in the world around him. The stability of the established order, social and economic, rests on loyal and contented acceptance by the average citizen of his station and its duties. He regarded the spirit of discontent as the enemy of the spirit of duty, and I believe that at the bottom of his heart he looked upon most of the social and economic unrest of our days as due, not so much to faults of the established order, as to the false moral values pursued by our generation. A philosopher may be forgiven for doubting whether the appeal of philosophy can ever reach the mass of men or move them so deeply as to remedy a spiritual discontent so wide-spread as that of our times. He is likely to feel even more impotent in the storm of conflicting human passions than does Mr. H. G. Wells's bishop (*cf. The Soul of a Bishop*) though the latter has the whole prestige and organisation of the Church on his side. If organisations so powerful as the Christian Churches prove incompetent to bring industrial peace, or to stop a world-war, or even to put an end to assassination in Ireland, what can philosophy and philosophers achieve? Yet, at times, Dr. Merz seems to have credited philosophy with this supreme power of bringing about the moral regeneration of mankind. There is, at least, one passage in the *Fragment* (p. 237) in which the task of defining the moral foundations on which human relations are to be organized is assigned by him to religion " for the earlier stages of civilisation " and to philosophy " in more advanced societies." Most probably this passage reflects the rare combination within himself of philosopher and industrial organizer. At any rate, Dr. Merz cannot be accused of having had a low opinion of the responsibilities and opportunities of a philosopher.

Let us, however, return from this extension of the task of philosophy to the moral sphere, to the tasks originally assigned to it, *viz.*, the " unification " of thought and, more particularly, the " reconciliation " of religion and science.

It will help us to appreciate Dr. Merz's own treatment of these two tasks, if we begin by considering for ourselves what are the different ways in which " unification " and " reconciliation," as applied to different thought-systems, may be understood.

Dr. Merz himself, in speaking of Comte (*History*, Vol. iv., p. 686), observes that Comte's " unity is essentially that of harmony; it is not a unity of thought or method, it is one of tactics or organisation." There are, then, several ways of unifying—indeed, there are, perhaps, even more ways than are here enumerated. Which of all these will supply the unification of which we are in search?

Let us consider.

(*a*) We have one type of unification when phenomena widely diverse in their *prima facie* character are brought under a common concept or law, as when—to use a familiar example from the Logic text-books—the breathing of animals and the rusting of iron are recognised as being phenomena of combustion no less than the burning of a log in the grate Other instances of this type of unification we may find, if Jacques Loeb is right in the application of the Roscoe-Bunsen law of photo-chemical reactions to heliotropic animals, or if Freud is right in the new psycho-analytic concepts which enable us to refer experiences so diverse as dreams and the countless slips and misfits of our everyday behaviour to the same mental mechanisms or, if Einstein is right in the way in which the theory of relativity accounts equally for facts which proved obstacles to the Newtonian theory and for the facts which fit well with that theory The distinctive character of this type of unification clearly consists in the extension of a known law or concept to a fresh group of phenomena, or, else, in the framing of a new law or concept bringing together and reducing to order facts hitherto scattered, isolated, chaotic

(*b*) A second type of unification may be illustrated by the work of modern "logisticians" or "mathematical logicians," *i e*, by such analyses as those by which the syllogism of traditional Formal Logic has been generalised and shown to be but a special case of a more general calculus, and those by which the mathematicians' concepts of space and number have been broken up into their ultimate logical constituents, until a new Logic has emerged which is the common foundation both of Mathematics and of what hitherto has gone by the name of Formal Logic, and in terms of the indefinables of which all the concepts can be "constructed" which mathematician or logician may need (*cf* Whitehead and Russell, *Principia Mathematica*) The distinctive characteristic of this type of unification is analytic reduction of complex concepts to simple and indefinable concepts, and, by a reverse process, deductive construction of complex concepts out of simple ones Whether this ideal of construction can be carried so far as to embrace the concepts, not only of mathematics, but also of the empirical sciences, may justly be doubted But there are thinkers who dream of an ultimate unification of all sciences in the sense of such an analytic reduction of the concepts of all higher sciences (*e.g.*, biology) to those of lower, *i.e.*, foundational, sciences (*e.g.*, chemistry and physics), and of these again to the concepts of sciences still more fundamental (*e.g.*, mathematics), until the base of the whole structure is reached in pure Logic.

(*c*) Of both the previous types we may fairly say, in Dr. Merz's words, that they are types of unification "of thought or method." But it is far from clear whether this description would apply to a kind of unification of which several varieties may be found in philosophical literature. Does Schopenhauer's Will, for example, provide a genuine unification, or Von Hartmann's Unconscious, or Bergson's *élan vital*? Or Herbert Spencer's Unknown and Unknowable Absolute? Superficially these examples may seem to exhibit some likeness to our type (*a*): they bring all things under heaven and on earth under a single concept. But there is a profound difference from the scientific type of unification (*a*). For, a scientific concept or law expresses a correlation of observable phenomena, and it does so, if at all possible, in a quantitatively determinate formula. These philosophical concepts, on the other hand, seek to name the reality which lies "behind" the phenomena. Hence, they presuppose a distinction between the appearance of things to an "outward" observer and their "inner" nature,[*] and generally they find the clue to the "inner" nature of the cosmos, to the reality behind all phenomena, in the "inner" nature of man himself, as revealed through introspection, intuition, or self-consciousness.

(*d*) Another type of philosophical unification may be taken as represented by Hegel's *Logik* and McTaggart's recent *The Nature of Existence*. Hegel believed that by a purely logical development he could build up the system of concepts ("categories") from Pure Being at one end to Absolute Spirit at the other. This system he regards as embodied in Nature and in History, in Science, Art, Morality and Religion. For him philosophy meant "phenomenology"—very literally a "theory of phenomena," but of phenomena as appearances of Absolute Spirit, each different kind of appearance having its determinate place in the hierarchy traced by his dialectical method. This kind of unification Dr. Merz, whilst paying an eloquent tribute to Hegel's genius ("we are compelled to regard Hegel's philosophy as one of the greatest, if not the greatest, intellectual performance of the century"—*History*, Vol. iv., p. 652), emphatically rejects. He quotes with approval Bradley's scathing criticism of the "bloodless ballet of categories," and elsewhere he speaks of Hegelian idealists as "evaporating all religious truth into mere abstract notions such as the Absolute" (*Fragment*, p. ix). In fact, friends and critics alike are agreed that Hegel's system, as it stands, is, in Dr. Merz's words, a "gigantic failure," however important may have been the impulse which it communicated to subsequent philosophical thought. Even McTaggart, the foremost expounder of Hegel in England at the present day, holds that the categories do not, in fact, stand to each other in the relations required by the dialectical method. Whether McTaggart's own attempt to do Hegel's work over again in a fresh way will fare any better, remains to be seen. The final judgment on this point cannot be spoken until the second volume of his *Nature of Existence* has appeared, in which we are promised the application of the *a priori* conclusions of the first volumé concerning the structure of the universe to the empirical world as we actually find it.*

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[*] The language in which this distinction is expressed may vary from one philosopher to another. A typical modern formula is Bergson's in the opening sentences of his *Introduction to Metaphysics*. There are, he says, "two profoundly different ways of knowing a thing. The first implies that we move round the object; the second that we enter into it. The first depends on the point of view at which we are placed and on the symbols by which we express ourselves. The second neither depends on a point of view nor relies on any symbol. The first kind of knowledge may be said to stop at the *relative*; the second, in those cases where it is possible, to attain the *absolute*" (p. 1).

(*e*) Lastly, the type of minimum unification may be found in all those theories which "reconcile" science and religion merely in the sense of showing that we need them both because both are grounded in human experience; that they can co-exist without mutual contradiction or conflict; and that religion may even be regarded as supplementing and completing the scientific view of the universe. The model for this type of unification may be found in Kant's account of the relation of " pure " to " practical " reason, *i.e.*, of the relation of the determinism of science, based on the concept of causal necessity, to the freedom of the will which is the postulate of morality. We may, perhaps, paraphrase Kant's position by saying that both science and morality are undeniable facts in our lives; that we must, therefore, hold fast to the fundamental principles of both; and that we can do so without contradicting ourselves if only we keep the scientific and the moral points of view completely distinct. On the other hand, Kant acknowledged himself unable to find any positive relation between these two points of view, and thus insisted upon the incomprehensibility of their co-existence. Not a very satisfactory solution, it will be admitted. For, dodging a contradiction is not exactly effecting a reconciliation. It is a little like ending the incompatibility of husband and wife by a divorce which makes them live apart and cut each other dead for the future. Yet the tactics of avoiding a contradiction by making a distinction are sound in principle. Hence, Lotze renewed the attempt by introducing a distinction between the world of " things " or " facts " and the world of " values," and he sought to make his reconciliation more positive than Kant's by showing that the world of values is realised, or is in process of being realised, in the world of facts. This method of unification is, clearly, of the sort called by Dr. Merz a unity of " harmony " or " organisation," viz., a harmony effected by insisting upon a fundamental distinction between two points of view which, else, would be deadly rivals. Lotze's skilful use of the method has been of very considerable influence on subsequent thought. It has supplied the philosophical basis for the whole school of theology of which Ritschl was the founder. It has found expression in the distinction between *Naturwissenschaft* and *Geisteswissenschaft* which, under the leadership of Rickert, is being elaborated by an important school of German thinkers In yet another direction it has given rise to the study of value-judgments, both in their psychological constitution and origin, and as supplying the bases for economics, æsthetics, ethics, and the philosophy of religion In fact, *Werttheorie* derives its origin from the problem which Lotze put when he distinguished between fact and value A closely allied way of harmonizing apparent incompatibles by a distinction is to connect science with *intellect* or *thought*, and religion with *feeling* or *emotion*, and to say that, since both intellect and feeling are founded in human nature, both have their rights We have as much right to judge the universe by the feelings which it evokes in us as by the way in which it presents itself to the dispassionate gaze of the intellect To omit the feeling-side of our nature would be to omit so to speak, one half of the available evidence

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* McTaggart's professed goal is to exhibit the universe to us as, in its ultimate character, a society of spirits. The attainment of this goal will certainly involve a cancelling of the differences which we ordinarily recognise between Matter, Life and Mind, such as departs widely from the maintenance of these distinctions in Hegel's ordered phenomenology.

Dr Merz's reconciliation of religion and science is without doubt of this last type and follows in its general character, the Lotzean model His statements of it contain explicit references both to the distinction of fact and value and to that of intellect and feeling But it is equally important to point out that Dr Merz introduces from other sources a number of fresh considerations which, in effect, make his position, not a mere restatement of that of Lotze, but a distinctive and in some ways original contribution to the philosophical discussion of the problem

The argument has two sides The first is negative it consists of a philosophical criticism of natural science, directed towards showing that science does not use the whole of our experience in the construction of its world-view and is, so far, deficient The second side is positive it seeks to lay bare the psychological roots of both science and religion, and thus to exhibit them both as natural and inevitable growths in the human mind And, further, it seeks to show that, precisely in virtue of its psychological roots, religion both supplements and embraces science

Dr. Merz's criticism of science may be summed up under the following four heads:—

1. Science "abstracts": "it rests . . . upon a process of selection, and in fact of very narrow selection, within the whole field of consciousness with its manifold experiences" (*Rel. and Sci.*, p. 88). It is just because science does not cover the whole of experience that there is room for religion. Moreover, religion does not simply stand alongside of science—as it were, parallel and independent. No, if science abstracts and selects, there is room for a synoptic, inclusive point of view, and such, precisely, is the point of view of religion. "The real nature of things as distinguished from that imaginary or abstract feature which we, in scientific research, substitute for it, reveals itself to us only if we look at it as a whole . . ." (*ibid.*, p. 103).

2. Science "dissects": its method is first analytic and then synthetic Religion gathers into itself the whole of our experience: its method is "synoptic"—"seeing things together."

3. Science seeks "descriptions" and eschews "interpretations." Otherwise put, it deals with "things" and ignores "values." Religion is our response, in worship and adoration, to the supreme values which a spiritual interpretation discerns in the world.

4. Science "depersonalises." Religion, at its best, is communion with a personal God.

These four criticisms of science may be condensed into one. They are all variations on the single theme that science is "abstract." What exactly does the term "abstract" mean when applied to science? The answer, as it may be found in Dr. Merz's pages, is cumulative. It cannot be given, so to speak, on a single plane. On the lowest plane, or in the most general sense, science is abstract because the objects with which it deals—we may sum them up in some such blanket-term as "physical nature," or the "external world," or the "material world"—are treated in isolation from the context of experience in which they actually occur, or, to put it differently, in abstraction from all that is "subjective," such as the feelings and interests they arouse, the activities to which they give rise, etc. Nature, as Professor A. N. Whitehead has neatly put it, is "closed to mind." Thus, in restricting itself to the "external" world, physical science leaves aside the whole "inner" world. It deals with "bodies," not with "minds." At any rate, there is a steady and powerful pull throughout all branches of natural science towards the methods and theories of mathematical physics. Hence—and here we shift to another plane of argument—science tends to deal even with the "external" world of nature, not as a whole or in all its aspects, but by preferring those portions or aspects of it which lend themselves to description in terms of classical Newtonian physics, *i.e.*, "in terms of Time (flowing in measurable lapses) and of Space (timeless, void of activity, euclidean), and of Material in Space (such as matter, ether, or electricity)."[*] Such a description fits inanimate bodies well enough, but hardly does justice to living, and still less to conscious, bodies. Certainly, the expression of minds through bodies the manifestations of feeling, thought and will where they occur, must needs be ignored by natural science. They call for interpretation, not for description. They do not lend themselves to mathematical treatment. They fall outside the framework of the concepts of physics and chemistry. Thus science has no room for the chief interpretative concepts of purpose and personality, either in the human sphere, or as applied to nature as a whole. And a further point on which Dr. Merz, following here Wilhelm Wundt, is fond of dwelling is the contrast between the "growth of mental energy" and the "degradation (or dissipation) of physical energy." Whereas the motions due to purely physical energy tend towards an equilibrium indistinguishable from absolute rest and death, the history of civilisation exhibits the cumulative creativeness of mind. "The great architectural structures to be found in ancient and modern countries, the collections of books, the galleries of painting and sculpture, the academies of learning, the houses of worship, the songs of the people, and the sublime creations of musical composers, testify to the existence of a world which is quite distinct from the amount of material or physical energy which has been used and expended in its creation" (*Fragment*, p. 192). This is the "World of Values" which is a creation of mind (*ibid.*, p. 230) and to which mind is constantly adding by its creative activity. Thus, Dr. Merz might almost have accepted Bergson's formula that the concepts of science fit "matter" and enable us to control it for our practical needs, but that they cannot deal with the creative *élan vital* which uses the human mind as the spearpoint of its advance. In all these several ways, then, science omits, selects, and thus "narrows the vision." Yet "even the student of pure science has always to turn again from his rigid calculation and measurement to the broad view of actual life: he has to take in at a glance the world as it is before he dissects it; he has to recognise that no analysis and subsequent synthesis exhausts the nature of any visible or tangible thing, that the *ensemble* is more than the sum of its parts" (*Fragment*, p. 196).

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[*] Quoted from A. N. Whitehead, *The Principles of Natural Knowledge*, part 1, "The Tradition of Science," ch. i., p. 1.

In this way Dr. Merz appeals to the critical reflection of the philosopher whom we must suppose to be lurking, though often severely repressed, in the bosom of every scientist. Nor should the point of these criticisms of science be misunderstood. Their aim, clearly, is not to discredit science as science, but to point out that, from the nature of its methods, it cannot be all-inclusive: it cannot deal with the world of our experience as a whole. If these criticisms seem pointless and irrelevant to any scientist, let him bear in mind that they are not the criticisms of an outsider: they are not inspired by sweet ignorance of, or unfamiliarity with, scientific ways of working and thinking. No, it is precisely the fact that Dr. Merz was originally trained as a physicist and chemist and that he spent his practical life in the application of science to industry, which gives to his criticisms a serious claim upon the attention of all scientists who are at all interested in the problem of the scope and the limitations of science. Moreover, the argument, so far, required that we should single out Dr. Merz's criticisms of science. But this does not do justice to the whole of his view. On the contrary, his writings are full of acknowledgments of the value of science, both in its logical achievements as theory and in its practical applications. The first two volumes of his *History* are in themselves a monument to the scientific spirit which he throughout acknowledges and acclaims as one of the most important and beneficient forces in the XIXth century. And to the careful and sympathetic analysis of scientific method he devotes, not only the section on "Science" in *Religion and Science*, but also a special chapter (ch. x.) of the *Fragment*. Thus, it is in no spirit of hostility to science that Dr. Merz emphasises the inherent limitations of its outlook and method. It would be truer to say that he dwells on these limitations just because he was so keenly aware from his own experience of the spell which science can exercise over the mind, both by its practical contributions to human welfare and by its fascination as theory. Its discoveries arouse our curiosity and stimulate our imagination. The rigour and precision of its mathematical methods appeal to our intellects. Alike its theories and its practical applications give us an almost intoxicating sense of power. When the spell of science is upon us, we are apt to forget its limitations and need to be reminded of them. Like Kant, Dr. Merz appears to have felt that he had to point out the limitations of science in order to make room for religion, meaning by "religion," as defined in *Religion and Science* (p. 2), "such convictions as refer to our Duty in relation to our Destiny as human beings."

The argument, then, is so far of the familiar type: harmony by way of distinction between religion and science, *i.e.*, by way of bringing out the distinctive point of view and method of each.

But, as we said above, this is only one half of Dr. Merz's case. The other half, which is his special contribution to the whole discussion, consists in tracing both science and religion to their roots in the human mind by a psychological argument which is at once introspective and genetic. It is, I think, for this combination of the introspective with the genetic methods that he would have claimed such relative originality as his view possesses. It seemed to him that the larger resources of modern psychology, especially as applied to the study of the development of the human mind from the first dawn of consciousness in the infant to the adult's full participation in the world, made possible a fresh and more fruitful use of the "introspective method," which he regarded as the most valuable contribution of the English school (Locke, Hume, Mill) to philosophy. Thus Dr. Merz's last word on the reconciliation of science and religion consists in showing us how they both develop in the human mind by differentiation from each other out of their common basis in what he calls the "primary," or "primordial," level of consciousness. In describing this level of consciousness with which all experience begins, he availed himself of James Ward's concept of an everchanging "continuum of presentations" and of William James's very similar concept of a "stream of consciousness." But he did not adopt these concepts without modification. He points out that the flow of thought, however undifferentiated it may be in the beginning, soon "seems to eddy round and encircle definite objects" (*Religion and Science*, p. 21), which recur again and again in perception and memory, which keep their identity throughout the changes which they undergo or the varying aspects they present, which come to have a fixed position in time and space relatively to other objects. Inasmuch as these objects thus stand out from the general background of consciousness, like stars against the expanse of the night-sky, Dr. Merz loved to speak, in a striking simile, of the "firmament of thought." It is with this concept of a firmament of thought that his synoptic method is especially connected. For, he holds that science arises through pushing further the abstraction and, as it were, isolation of the outstanding objects from the background, defining them more precisely, determining their relations to each other in space and time, formulating laws of cause and effect for their changes. Yet, on the other hand, these same objects remain steeped and immersed in the whole context of experience from which science abstracts them. They evoke feelings, interests, activities which science ignores. It is the function of the synoptic method to restore the context destroyed by abstraction, and religion, more particularly, is the response of our whole minds to the whole of reality. For, religion, just because it is synoptic, means " being absorbed in, and at one with, the whole " (*Fragment*, p. 53).

But, if we have followed Dr. Merz so far, a formidable problem is still left over and must now be confronted.

The problem may be put thus. Even sympathetic readers and critics will probably agree with me when I venture to say that Dr. Merz's argument is more convincing on its negative than on its positive side. It is not difficult to go with Dr. Merz when he points out the limitations of science. Even a devoted student of science, it seems to me, will not, if he at all reflects critically on the methods and assumptions of science, deny these limitations. Indeed, he has no reason to deny them. For, to point out these limitations is not to depreciate the value of science: on the contrary, the limitations of science are acknowledged to be also the conditions of its success. The argument is not that science is a failure in what it attempts by its methods: the argument is that there is something more in human experience which science does not, and with its methods cannot, attempt to deal with. And so there is, again, not much difficulty in agreeing with Dr. Merz that the sense of this something " more " which is brought home to us by our experience as a whole, is the root of religion—using the word in a very general sense. The difficulty, it appears to me, begins when we come to define positively what this religion is which is thus to be reconciled with science. Granted that Dr. Merz has successfully answered his own question: " How is Religion possible? " (*Fragment*, p. ix.), it is hard not to feel that the full task of reconciliation is incomplete, unless we are shown also *what* are the positive doctrines of this religion which is to be compatible with science. If a "reasoned creed" is the goal of our quest, what precisely are its articles? In one passage, as we saw, Dr. Merz speaks of the unification of scientific thought with "Christian truths." But what, precisely, are the essential Christian truths, and how far can they be established by an introspective and genetic study of the development of individual minds in social intercourse with each other? Considering that Dr. Merz wrote the essay on *Religion and Science* with the explicit purpose of helping "thoughtful" members of the younger generation "who feel themselves sore perplexed by the contradictions which apparently exist between the dicta of science and the tenets of the religious creeds, who are not prepared to sacrifice the truth of either, but who find it extremely difficult to reconcile them" (p. 4), I, for one, cannot but feel some regret that Dr. Merz did not dwell on the positive side of his argument with greater fullness and detail. Thus, *e.g.*, a great many persons who review their religious beliefs in the light of their scientific knowledge, find themselves unable to accept, as historical facts, such things as the virgin-birth, the resurrection, and the ascension to heaven. For, if they were to accept them, they would have, *e.g.*, to believe, with the Rev. Father R. A. Knox, that at the ascension the total weight of the earth was actually diminished by the amount of the weight of Christ's body. And, in general, scientifically-trained minds have difficulties with all the miraculous stories in the New Testament, or, if they cease to have difficulties, it is precisely so far as these stories can be matched, like the healing miracles, by the faith-healings of our own days, *i.e.*, precisely so far as the miracles of the Bible cease to be "unique" and "without parallel," and promise, by assimilation to phenomena which we can observe and experimentally repeat, to come within the range of scientific explanation. Yet it is by these two marks that Dr. Merz defines a miracle: it must be "incomprehensible" and it must be "exceptional" (*Rel. and Sci.*, p. 188). Well, then, taking "miracle" in this sense, what argument does Dr. Merz offer to reconcile acceptance of miracles with our scientific knowledge? The answer is that science abstracts from the "personal" element in experience, that the manifestations of personality, even of merely human personality, are largely indefinable and incalculable by the methods of science, and that, therefore, if the personal element be admitted, the mysterious and miraculous cannot be excluded (*Rel. and Sci.*, pp. 188-191). I must frankly confess that this argument leaves me unconvinced. My difficulty is not so much that so general a defence of miracles—a defence of miracles as, in principle, possible and to be expected—leaves the report of any given miracle still open to doubt For, this is merely to say that the argument, from admitting the principle, would now have to shift to the credibility of our historical record for each separate miracle reported. It certainly would not do from the premiss that the manifestations of personality are incalculable to infer that, *e.g.*, the virgin-birth is a fact. No, my real difficulty with the argument is that the sense in which the actions of persons are for science incalculable and unpredictable is not a sense which covers such a miracle as the feeding of the 5,000 or the ascension. The miracles of personality of which Dr. Merz is really thinking consist in the creativeness of human minds, as exhibited, *e.g.*, in the composition of a symphony or the writing of a poem. Again, we may, if we like, call it a "miracle" when men rise in a crisis to unexpected moral heights, or display their inventiveness and originality in solving practical problems. It is easy to grant that science—and especially physical science—cannot explain these things. But they are not, therefore, strictly either "incomprehensible" or "without parallel." In fact, in the sense in which Dr. Merz defends miracles, every manifestation of mind, however normal or familiar, is miraculous. But the sense of miracle which creates difficulties for scientifically-minded people is the sense of miracle as a unique historical event due to the exceptional interposition of God.

Indeed, reviewing the whole argument, we can hardly regard it as insignificant that Dr. Merz does not discuss, or even mention, any single one of the miracles in the Bible-story. He makes no attempt to show how he would apply to them his principle that belief in the miraculous is implied in belief in a personal God. The conclusion is almost inevitable that, whilst belief in God was the very centre of his thought-world, the particular miracles of the Bible-story play a wholly negligible part in the evidence on which he, personally, based his belief.

This is in keeping, too, with his use of the term "revelation." By "revelation" Dr. Merz means, quite deliberately, not special, miraculous communications from God on certain historical occasions, but the constant and continuous manifestation of spiritual reality through sensuous form. Thus, it is "revelation" when we first learn to interpret the behaviour of human "bodies" as expressing the feelings and thoughts of other "persons" towards us. It is "revelation" when we interpret "our ever-recurring feeling of dependence with its characteristic sphere of emotions, the foremost of which are fear, reverence, and love" (*Rel. and Sci.*, p. 190), as the working upon us of a "Highest Spiritual Power." It is "revelation" when we trace in the course of History the gradual ascent of humanity to higher moral standards and a fuller knowledge of God.

Apart from miracle and revelation, there are two theological topics which Dr. Merz briefly discusses in *Religion and Science*. The first is the way in which we come to believe in a personal God. The second is concerned with our conceiving this personal God as both immanent and transcendent.

The arguments on these two topics are closely connected together, and may be summarised as follows:—

The first objects which come to stand out definitely in the "firmament of thought" are persons, *i.e.*, bodies to which we attribute the same sort of "inner life" as we are aware of in ourselves when, synoptically, we take in the whole of our firmament of thought. To recur, by such synoptic reflection, from a definite object to the whole firmament of thought, is to make ourselves aware that what we mean by the attribute of personality is an "all-embracing something." It signifies, as Dr. Merz boldly puts it, "completeness, in fact the All" (p. 171). Yet we are conscious, at the same time, of the incompleteness and fragmentariness of our own personalities. Thus we are led to think of the universe, which confronts us and which yet also includes us as parts, as the manifestation of a much more comprehensive personality than we experience in ourselves. Thus "personality," as it is the first thing we learn to distinguish, so it is also the last and highest term we can use in the attempt to make intelligible to ourselves our "feeling of dependence," our sense of the "spiritual pressure" (p. 176) upon us of a Greater-than-ourselves, of which we are members even whilst, as greater, it is beyond us, absorbing our incompleteness into its perfection.

"And I smiled to think God's greatness
Flowed around our incompleteness,
Round our restlessness his rest."

The experience here poetically rendered becomes, translated into the technical language of theology, the doctrine of God as at once immanent and transcendent, a spirit working at once within us and beyond us.

When we turn to the *Fragment*, these theological problems drop into the background, and, instead, the treatment of religion is dominated by emphasis on the moral law, conceived as grounded in the Will of God. Indeed, it seems to me that in the chapters on "Philosophy and Religion" and on "Revelation" in the *Fragment* (chs. xv, xvi). Dr. Merz has succeeded better than anywhere else in laying bare the ultimate roots of his faith. The greatest spiritual need of man, so I would paraphrase and summarise his thought, is a rule of conduct based on a view of the totality of things, *i.e.*, of "reality," as Divine. The stability of social life rests on the individual's loyalty to moral ideals, and if he is to trust these ideals and to be faithful to them even when such loyalty demands effort and sacrifice, he must look upon them as expressions of the Will of God, and upon God as the ultimate reality. The task of philosophy, in furnishing a " reasoned creed," is to exhibit this dependence of the social order on morality and of morality on religion, and, further, to justify (if I may call it so) religion by showing how it is rooted in human experience and is our response to the recognition of the spiritual character of reality. But a new, and most important, note creeps into the argument Not only must our approach to religion be synoptic,* but we must always remember that the condition for understanding religion is to be religious. This, I must warn my readers, is my own way of putting the new and marked emphasis in this last of Dr. Merz's works on " living faith " and direct " personal experience " of religion and of the truth of the religious interpretation of the world " The fact that Humanity has received what professes to be a highest and unalterable rule of life and a name for the highest Reality, both being identified in the Christian conception of Love, is a fact which must be contemplated as a whole, and, as such accepted in faith or rejected. There is for those who accept this Revelation only one other proof possible, and that is their own experience of the workings of this faith in their own lives and in those of their fellow-men. No philosophical reasonings, no historical criticisms avail either to generate or to destroy this faith—it stands and remains as a fact by itself with no parallel in the whole range of other experience " (*Fragment*, pp. 292-3). There are passages in which Dr. Merz expresses this very sound point by saying that " no logical proof is possible " of the Christian view of life (p. 269), that it cannot even be understood " by purely logical forms of thought " (p. 280), nay even that it is " irrational " and " only to be seen or experienced " (p. 288). What an apparent paradox, if we take these phrases literally, that the quest for a reasoned creed and a unification of all thought and knowledge should culminate in proclaiming religion to be both irrational and the highest thing in life Of course, all depends here on the restrictive meaning to be attached to terms like "pure logic" and "reason" Thought or reason can "prove" nothing without a "matter" or "content" which experience only can supply Or, to put this quite bluntly and simply We must have *something to think with*, else our thinking is empty and our terms are meaningless We shall labour in vain to prove by pure logic to a person congenitally blind the difference between green and red, or to get him merely to understand what we mean by the terms "green" and "red" Lacking normal sight, the words "green, "red" "colour' are simply sounds to him But, given sight the terms have a meaning and can be used in intelligent and intelligible, reasoning we now have something to think with *Mutatis mutandis*, this is, it seems to me what Dr Merz is here saying about religion You cannot "prove" religion to a person who has not the kind of first-hand experience of or acquaintance with, religion which we call quite simply "being religious" In the absence of it the very term "religion" has hardly any meaning for such a one, unless the meaning be derived from, and restricted to what an observer can perceive of the outward words and bearing of religious people But that is certainly to miss the inner spirit Hence, religion is not so much irrational, as that it can be reasoned about only by, and between, those who, being religious, know what they are talking about This is, I think, the real importance of Dr Merz's emphasis on "experience" as against "purely logical" proof

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* *Cf*. the following striking passage concerning the "great structure of Christian Thought and Christian Life"; "The contemplating mind stands before it as before a great Reality which can be judged fairly only in its wholeness, but which like all great things threatens to crumble to pieces if we look only at single data, facts, and events, and try laboriously to put them together into the totality of a logical structure" (*Fragment*, p. [illegible]).

To sum up Dr Merz's attitude towards this whole problem of science and religion is, I believe, best described in the familiar words, *fides quærens intellectum* "faith seeking understanding," or, more clearly still, "faith seeking to understand itself" It is not that understanding must come first, and faith follow Faith comes first, standing secure on its own foundations, and the function of "understanding," or philosophy, is to raise this immediate and spontaneous security to the level of reflective, self-conscious assurance. I am unable, therefore, to agree with Dr. Jevons when, in his most interesting paper on "A Synoptic Philosophy,"* he criticises Dr. Merz for having treated the religious world-view merely as an *hypothesis* which requires intellectual justification and proof, and then goes on, rightly enough, to urge the profound difference between the assurance of faith and the tentativeness of an hypothesis. There are, no doubt, passages, especially in the *History*, which, whilst hardly going so far as to treat religious belief as an hypothesis, may yet be read as making acceptance or rejection of religion wait upon the issue of a philosophical argument. But the *Fragment*, I submit, puts Dr. Merz's real position beyond all doubt. Dr. Merz does not ask, "Is religion possible?" He asks: "*How* is religion possible?" He accepts religion as a fact, and a *necessary* fact, and his concern is only to exhibit that necessity, to "understand" it, by tracing it to its roots in human experience. We must always bear in mind that the audience to whom Dr. Merz addresses his argument is one ready to accept science as certain but inclined to treat religion as doubtful. Of his own life and thought, on the other hand, religion was undoubtedly the foundation. Hence, all his philosophy is, at bottom, *fides quærens intellectum*.

III.

In dealing with Dr. Merz's contribution to the reconciliation of religion and science we have, inevitably, touched already on some of the technical problems which are to form the topic of the second part of this review of his philosophical work.

1. There is, first of all, Dr. Merz's *synoptic* method, which is the apprehension of wholes *as wholes*, in their total effect: a *vue d'ensemble* for which no scientific analysis,

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* See the *Church Quarterly Review*, vol. lxxxii., No. 164 (July, 1916). The full title of the paper is "Science, Ethics and Art: A Synoptic Philosophy." Dr. Jevons has dealt with Dr. Merz's philosophical views also in another article, viz., "The Pursuit of Reason," in the *Edinburgh Review*, No. 436 (April, 1911).

followed by synthesis affords an adequate substitute, which is akin to the artist's vision and which is the very essence of the philosopher's contemplation of all time and all existence. The term synoptic, in fact, is borrowed from Plato, and thus helps to remind us that the method is as old almost as philosophy itself. It is kindred to Aristotle's *νοῦς*, to Spinoza's *scientia intuitiva*, to Kant's *reason* which reaches out beyond the unending series of phenomena towards the all-inclusive whole. It links Dr Merz, too, in spirit with Hegel. Rather than describe in inadequate words of my own what synopsis means to Dr Merz I will quote one of many passages from his writings: "The real nature of a fact ... is only revealed through a comprehensive glance which gathers up all the single features and all the many instances which form the substance of manifold and often repeated experiences into a collective view which in some indescribable manner conveys to us something deeper and more responsive than any or all the single features added together. ... In this way, for instance, the minute study of a work of art may all of a sudden brighten into a real understanding of it and admiration rise to rapture and enthusiasm; so also the acquaintance with a fellow being may after frequent intercourse ripen into friendship and the pleasure of repeated meeting and conversation burst into love. We feel instinctively that the refined and spiritual experiences do not belong to objects or persons in their purely spatial existence but form a world for themselves in the same way as in the purely intellectual region numbers, mathematical formulæ and logical conceptions form realities by themselves." (*Fragment*, pp 184-5.) Synoptic power likewise seemed to him to distinguish the minds of men eminent as organizers in practical affairs. In practice as in theory synopsis brings order out of chaos precisely because it "sees things together." I find this expressed among the fragments of the "New Philosophy" which Dr Merz dictated during the last winter of his life. "Synoptic minds," he there says, "deal with the whole and not only with a restricted portion of human experience. They do not, as a rule, discover or invent new things, but they add to the existing experience of themselves and others a new feature. This new feature is Order, a process of extensive grouping. They take synoptic views, discarding the analytical and criticising method. Their aim is construction, arrangement, and control. They are struck by the fact that the contents of the human mind as given by nature are chaotic, as are likewise the unregulated occupations of practical life."* It is, clearly, the synoptic quality of the artist's vision which led Dr. Merz to affirm, again and again, that "art stands in closer relation to Nature than science does," that it comes "nearer to the real essence of things, to the kernel of reality" (*cf. Frag.*, p. 224, and *Rel. and Sci.*, p. 139).

2. The most remarkable feature, however, of Dr. Merz's use of the synoptic method is his identification of it with the introspective and genetic methods of psychology.† The concept of "primordial consciousness" as a *continuum* of presentations is put forward by him as a result of synopsis on the twofold ground (*a*) that, in recognising the "together" or "stream" of sensations, modern psychology has abandoned Hume's analytic method which, on the analogy of physical atoms, had broken up consciousness into a "bundle" of unrelated, atomic sensations, and (*b*) that it includes in the *continuum* emotional and volitional as well as sensational elements. He acclaims the discovery of this primordial consciousness as "one of the principal results of philosophical research" (*Rel. and Sci.*, p. 79), and in the *History* he speaks of the *introspective* method as affording the most hopeful prospect of carrying out the unification of thought which is the task of philosophy (p. 773). This is explained later (pp. 784-5) as meaning that, by using the introspective method genetically (as Locke and Hume had done) we can trace how each mind, beginning with the undifferentiated *continuum* of sensations and feelings, learns, largely with the help of

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* For an earlier statement of this point, see *History*, vol. iv., pp 775-6.

† *Cf.* "This looking at wholes is our mental attitude when we take the introspective view" (*History*, vol. iv., p. 777).

other minds (through language, imitation, etc.), to construct the "outer World of Things," and, subsequently, "through the co-operation and successive labours of the more highly gifted minds, the World of Values, of Truth, Beauty and Goodness."

I must frankly confess that the transitions of thought in Dr. Merz's argument here have always proved to me somewhat baffling. I can understand the sense in which Dr. Merz contrasts Ward's *continuum* of sensations, feelings, and strivings, as "synoptic," with Hume's mere *bundle* of sensations and images, as "analytic." But, in other passages, Dr. Merz himself describes this primordial continuum as a "chaos"—he quotes and adopts William James's famous description of it as "one great, blooming, buzzing confusion" (*Rel. and Sci.*, p. 63)—whereas synopsis reveals, or creates, ordered, organized wholes. Thus, the transition is by no means clear, and, if we are not to say that Dr. Merz uses "synopsis" here in different senses, we must at least say that the synoptic method is exceedingly many-sided, and that in different contexts Dr. Merz uses different sides of it. Certainly synopsis requires in one context the inclusion of all experiences in one view (as against abstracting selection); in another context it requires aliveness to relations and connections (as against dissection and separation); in a third, it finds, or makes, order and builds up wholes out of chaotic material; in a fourth, it is the flash of insight which reveals the single, total meaning of what was up to then a dead mass of details. Perhaps the combination of all these things, and more, in the single notion of synopsis is itself an instance of synopsis of the highest order.

However this may be, Dr. Merz is right in the importance which he ascribes, alike for psychology and for philosophy, to the concept of "primordial consciousness." It is, I am sure, not merely fanciful to affirm a certain affinity between his thought and that of a contemporary who is justly acknowledged to be the greatest metaphysician among recent English thinkers. Mr. F. H. Bradley's concept of "immediate experience" is identical with Dr. Merz's concept of "primordial consciousness." For Mr. Bradley, as for Dr. Merz, the undifferentiated "Together" of immediate experience is broken up by thought, with its "ideal constructions" of "things," "self," "other," "body," "soul," "outer" and "inner" world, etc. Mr. Bradley's world of ideal constructions reposing upon the background of immediate experience is, thus, the analogon of Dr. Merz's "firmament of thought," with its definite objects standing out from an undifferentiated background. And when Mr. Bradley declares that the highest form of experience must combine the order and organisation of thought with the immediate wholeness of feeling, he links the lowest and the highest levels of experience much as Dr. Merz makes the span of synopsis cover the introspective awareness of the primordial continuum with the insight which enables artist and philosopher to grasp a whole as a whole "at a glance."

3. This parallel is the more remarkable because there is no evidence that Dr. Merz, though acquainted, of course, with Mr. Bradley's writings, was directly influenced by them. More probably such likeness as there is must be traced to the influence of the psychology of their time upon both. In any case, it is certain that, beyond this general resemblance, Mr. Bradley and Dr. Merz diverge widely in their method and views, and that Dr. Merz looked to Locke and Hume, not to any contemporary, as his masters in method.

The particular feature of Hume's method which seemed to him valuable was the technique of tracing every abstract term back to its foundations in concrete experience. Take such philosophical terms as substance, cause, subject, object, self, not-self, matter, mind, etc.:—if they have any meaning, it must be in virtue of some actual element, or elements, in experience which they express. As Hume puts it: every "idea" must have its original in some "impression" or else be meaningless. Dr. Merz generalizes this method by asking:—What is it in our experience that a given term means or expresses? To this question, so he held, the answer can be given only by going back to primordial consciousness as the starting-point, and then tracing, genetically, how with the development of that consciousness into awareness of, and response to, an articulate world the meanings arise which these terms fix and express In this light, we can best understand the central position which he assigns to the concept of primordial consciousness (or "firmament of thought") as well as to the genetic form of the introspective method Of the primordial consciousness he bids us think as, initially, wholly undifferentiated, as all "on the same plane," as it were It is a stream of sensations, feelings, impulses in which, at the very first no definite, self-identical objects stand out, no terms and relations, no subject *versus* object, no matter *versus* mind The initial plane of consciousness is, as we might say, "neutral to these and all other distinctions Distinctions and, through them, order and organization, come with the development of our minds as they accumulate experiences, until the mature mind lives on several planes of consciousness at once, or, as Dr Merz came to express it in the *Fragment*, recognizes different "orders and 'degrees of reality

There is no need to trace Dr Merz's handling of this general type of argument into all its details It will suffice to illustrate it by its application to the origin of (*a*) the various meanings of 'reality" and "existence', (*b*) the distinction between the "inner' and the "outer" world, including the incidental distinction between mind and body, (*c*) the distinction between "self" and "not-self"

One general observation must be made about all the arguments from which our illustrations are to be drawn As they stand in the pages of Dr Merz's writings, they all unmistakably exhibit that fragmentariness and lack of finality the reasons for which have been indicated earlier in this paper The general drift of the argument is always clear, but there are a good many differences, amounting occasionally to inconsistencies, between different passages, both in terminology and in doctrine Every one of our three topics has been discussed in several, more or less widely scattered, passages in Dr. Merz's books, and when these passages are brought together, they frequently strike the reader as so many independent attempts to wrestle with their problems. Points mentioned in one passage are ignored in another or stated with different emphasis and in different language. It is as if Dr. Merz, not having attained finality in his views, had never been able to express his *whole* mind on any of the three problems at any one time. Thus his whole mind has to be collected from all the passages together by a sympathetic and synoptic study. I feel sure that this is the spirit in which Dr. Merz would himself have wished his arguments to be treated. Hence, I shall not dwell on such discrepancies in detail as there may be, but try, as far as I can, to reconstruct his whole thought at its best.

4. We begin, then, with (*a*) the various meanings of "reality" and "existence."

The fundamental principle from which Dr. Merz starts is thus formulated in the *Fragment* (p. 39):—

"All knowledge, of whatever kind it may be, is contained for every individual person within the range of his own consciousness. The horizon of any person's mind contains everything that exists so far as he is concerned. There is nothing in the world for any of us but that which we in some way or other mentally experience—such experience being of various kinds, such as Sensations, Perceptions, Ideas, Emotions, Desires, Volitions or Feelings in general."

This doctrine, at the first blush, may seem simple and plausible, but it is neither simple nor plausible without the most careful interpretation. A Realist, for example, might urge that it commits Dr. Merz to Berkeley's view that *esse est percipi* (to exist is to be experienced), and that, consequently, nothing exists except when and so long as it is "contained within someone's consciousness." I do not think this is what Dr. Merz means, nor have I found any passage in his writings (though there are some incautious phrases) which demands the Berkeleian, and excludes every other, interpretation. I would urge that the emphasis in Dr. Merz's doctrine falls on the words *for* me, *for* any of us. To say that if a thing is to "exist for me" it must appear within the horizon of my consciousness, means merely that I *cannot know* of its existence, or that I *have no evidence* for its existence, unless it so appears. It does not mean that the thing cannot exist without being perceived. In short, I read Dr. Merz as defining, not what existence consists in, but what evidence for existence consists in In technical language, I treat his doctrine as epistemological, not as ontological.

Further, a thing can be "for me" or "contained in my consciousness" in more ways than one. I may, *e.g.*, either *perceive* it by my senses, in which case we are wont to say that the thing "itself" is "present," or I may *think* of it, as when I think of an event which is past and can no longer be perceived or of an event which is future and still remains to be perceived. Thus, we must distinguish between two sorts of "transcendence." There is transcendence of perception, viz., by thought, as when we think, and believe in the existence, of things not now perceived, and perhaps by their very nature not perceptible (*e.g.*, the minds of others). And there is also transcendence of all experience, *i.e.*, of both perception and thought. The former sort of transcendence Dr. Merz recognizes and includes in his doctrine (*cf. Rel. and Sci.*, pp 81, 2). The latter kind of transcendence is self-contradictory, at least if it means that there are, *i.e.*, that we know there are, things which we have never perceived or thought of, *i.e.*, of the existence of which we have no evidence whatever. It is, of course, quite a different matter, once we have had evidence of the existence of a thing, thereafter to think of it as one that exists even at times when we do not perceive or think of it. These points having been cleared up, we must next note that "existence" is ambiguous. Fundamentally, according to Dr. Merz, it means presence, or occurrence, as an experience within the total "firmament of thought." In this most general sense, the objects of normal perception and thought and the objects of dreams, illusions and hallucinations all alike "exist" or are real.[*] There is no distinction, so far, between objects which are "private" and objects which are "public," *i.e.*, common to several observers, or between objects which are in an eminent sense "real," and objects which are "unreal" and which exist, as we popularly say, "only in our minds" or "only as ideas."

The "real world," *par excellence*, of ordinary practical life and of science, as distinguished from the world of dream, fiction, make-believe, insane delusion, etc., arises by a differentiation within the field of primary existence which is co-extensive with the field of primary consciousness. Thus, we have two "orders," or "levels" of reality, of which the second is a development by selection of certain elements of the first. Dr. Merz sometimes also speaks of the second as an "objectification" (*Rel. and Sci.*, p. 26), or "projection" (*Frag.*, p. 90), of a portion of the first, but these terms have highly misleading associations. The two important things to remember are (*a*) that the secondary, or higher, reality—which we may also call the "external" and even the "physical" world—remains rooted in the "primary" reality which remains always accessible to a synoptic view, undoing the abstraction involved in the selection; and (*b*) that all phrases like "acquiring objectivity" merely mean that the objects of certain groups of experiences are treated by us as "real," not merely in the sense of "existing for us," *i.e.*, of being perceived, but in a special further sense.

This further sense is defined by the criteria which we apply in making the distinction. In his earlier statements Dr. Merz dwells chiefly on the criterion of "publicity" (as we may call it).[†] An object which is real in this eminent sense is one common, actually or potentially, to a multitude of percipients. If the perceptions of others corroborate mine, then the object I perceive is "real" in this eminent sense. In the *Fragment* (p. 122) the list of criteria has grown to four: (1) publicity; (2) definiteness or individuality; (3) position in space;* (4) membership of an ordered universe. In the MSS. of the "New Philosophy," a somewhat different list of five criteria is given in the following order: (1) recurrence in experience, permitting recognition; (2) definiteness; (3) detachment; (4) publicity; (5) membership of an ordered system. Very obviously, Dr. Merz's thinking had not reached finality in this point.

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[*] Dr. Merz most commonly uses "being real" and "existing" as synonymous terms. Existence in this fundamental sense has, of course, no negative. We cannot say of anything in consciousness that it does not exist (*Fragment*, p. 60).

[†] The term is not Dr. Merz's. What is here called a "public" object is described by Dr. Merz as "common" to a plurality of observers.

The theory, moreover, proliferates in various directions, tentative extensions of it occurring in several passages. Thus, in one passage the original two levels, or "dimensions," of reality grow into four, by the addition of the world of abstract mathematical entities and natural laws, and of the world of artefacts, *i.e.*, of things made for use or beauty (*Rel. and Sci.*, p. 140). Elsewhere, there is a sketch of an "order" of relations, *viz.*, Distance, Order and Number (*Frag.*, p. 123). Again, there is the world of values, truth, beauty, goodness and holiness, which Dr. Merz presents in one passage as but a special development of the inner world (*Frag.*, p. 133). Yet in another passage he places values as the third and highest form of existence on the top of the two dimensions with which we are already familiar. I quote: "Existence for us has three distinct phases: mere Existence as a mental experience, Reality, as a double or more pregnant form of existence, and Value as the highest form of existence" (*Frag.*, p. 65). The whole of chapter vi. of the *Fragment*, entitled "Of Reality in General," is full of unfinished attempts or suggestions towards a theory of both "degrees" and "orders" of reality, in the course of which we read that "the most real thing of which we have any conception or immediate experience is a person" (p. 108). It is to be greatly regretted that Dr. Merz had to leave his treatment of this very important, but also intrinsically very difficult, topic in a condition so fragmentary. My own belief is that, in trying to thread his way through the maze, he followed the wrong clue in that he identified the distinction between inner (or mental) and outer (or physical) with the distinction between what exists (but may be unreal) and what is real, and, again, with the distinction between what is subjective and what is objective. Dr. Merz, on the whole, treats these three distinctions as coincident, as merely differing in the terms by which they name the same two dimensions or orders of our experience. There is, I believe, more to be said for the view that these distinctions do not coincide, but belong to different contexts and are made on different grounds. The introspective and genetic approach to these problems which Dr. Merz adopts from psychology and handles with such skill, here proves misleading in spite of its initial promise. For it makes Dr. Merz characterize the primary consciousness as "inner," "mental," "subjective," before the distinctions between "inner," and "outer," etc., on which these terms depend for their very meaning, have arisen. Correlative terms, like these pairs, have meaning only in contrast with each other. If you take away the one, can you still significantly employ the other? I venture to suggest that the kind of theory at which Dr. Merz really aimed, and which he sought to outline in his constructive writings, has been, and is still being, worked out, more systematically and substantially, in the *Gegenstandstheorie* of Meinong and the *Phænomenologie* of Husserl. It is, at least, significant that both these thinkers, like Dr. Merz, were profoundly influenced at one stage of their studies by Hume, but it is significant, too, that they both abandoned the psychological point of view and its terminology to which Dr. Merz pinned his hopes. I feel sure that if Dr. Merz had become acquainted with the mature work of these thinkers, he would have recognized, in spite of the difference of methods, its kinship with his own endeavours.

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* But what of the spaces of dream-worlds and fiction-worlds, *e.g.*, the space of Alice's Wonderland?

5. In the course of this discussion of Dr. Merz's treatment of "existence" and "reality," we have had to touch, in anticipation, on a good many points which are relevant,

too, for our next topic, viz., (*b*) the distinction between "psychical" and "physical," or "outer" and "inner."

We cannot do better than begin the discussion of this distinction by considering a paradox on which Dr. Merz delighted to dwell. When we survey the panorama of the outer world, minds appear, here and there, as attached to living bodies, human and animal. They are, thus, sporadic and, when measured by the scale of cosmic space and time, insignificant phenomena. "Early instruction in childhood has taught us to look upon ourselves, including the whole of our field of consciousness, as units among a great number of other persons; upon the whole of the human race as one only among the innumerable specimens of animal creation; and upon the whole of this as a very small portion of terrestrial phenomena. Still further—our planet itself is only one in an innumerable crowd of other worlds, in which it almost disappears through insignificance. The whole of this is comprised in the still more overwhelming conception of immeasurable space which embraces, as it were, everything. This process goes more and more to convince us of the unimportance of experiences which belong to each one of us as a private possession, and forces us to assume that those uniformities which have been discovered in the all-embracing universe of space, so far as it is accessible to our observation, must be the primordial and highest laws of èxistence. We have thus two distinct worlds or orders of existence to deal with. The first is the entire stream of consciousness or the changing firmament of the soul: it contains, as a very small portion only, those elementary sensations of sight, touch, and sound, out of which common-sense builds up the external world, and science, with a still greater restriction of fundamental data, its edifice of methodical thought, its picture or model of the universe. We have, secondly, this external world in which our own person, including our entire stream of thought, appears as a mere speck. And it depends upon the position we take up whether the first or second of these existences impresses us as possessed of the fuller amount of reality. Each contains the other within its circumference, and is itself contained in the circumference of the other."* There are, thus, two contrasting points of view. From the one, minds are insignificant incidents on one of the meanest of the planets. From the other, all nature, from the farthest star to the nearest stone, is but an object selected by abstraction from amidst the continuous stream of experiences which is, for each of us, mind.

The link between these two points of view is, for each of us, his *body*. It is one of the earliest objects to be singled out from the stream of consciousness and to acquire "reality" as a member of the outer world, an item in the spatial system of physical nature. On the other hand, as a "cluster of sensations," it forms a permanent, and more or less prominent, ingredient in the stream of consciousness. In Dr. Merz's own words:—"We see ourselves as it were from two sides, first, as the totality of our present and remembered experience; and, secondly, as a definite assemblage of vivid sensations, which occupy only a very small portion of the whole field embraced in the first view. Our Self is thus compounded of two selves, an inner and an outer self. The first is the firmament of our thought; the second is our body. In contrast to the latter we call the former our Mind, and both together our Personality" (*Fragment*, p. 69).

Let not this language about "being compounded of two selves," or about mind and body "together" being our personality, be misunderstood. It does not mean what is ordinarily understood by saying that man is made up of a mind and a body somehow conjoined. The ordinary view implies that mind and body are distinct and separable things: indeed, death is thought to be precisely their separation, the breaking of the bond that holds them together in this earthly life. It is implied, further, that each can exist after this dissolution of their union, or, at any rate, that the soul can: indeed, the continued existence of the soul (which, surely, is nothing other than what Dr. Merz calls "mind") after its separation from the body is precisely what is ordinarily called "immortality" or "life after death." To this popular conception Dr. Merz's view obviously lends no support. It must, indeed, be admitted that he nowhere in his published writings discusses death and the belief in immortality, and thus we can only conjecture how he would have applied his theories in this field. That he disbelieved in immortality, we have no reason to suppose: in truth, such a supposition would ill fit his profoundly religious nature. But it is obvious, at any rate, that his declared theory of what mind and body are must have carried with it a conception of immortality widely different from the popular one. How, for example, should we have to interpret, on his view, the separation of mind and body at death? It can mean only the disappearance from the total consciousness of the special "assemblage of vivid sensations" called the body, but Dr. Merz's silence gives us no help in speculating upon the consequences of the elimination of so central a constellation, as it were, from the "firmament of thought." It must, one would think, carry with it a profound alteration, if not a diminution, of the whole personality, and thus raise awkward questions of continued identity. However, it is vain to pursue these guesses, fascinating as they are. We must accept the fact that Dr. Merz's interest in the concepts of mind and body takes a different direction. He keeps strictly within the scope of introspective psychology, from which the problems of death, and of the continuance of mind after death, are inevitably excluded. For, how can introspection possibly deal with death? It presupposes life and, indeed, self-conscious life. Even if death be an experience, an event in the flow of consciousness, rather than the final cessation, or the temporary interruption, of that flow, still it ruptures communication with those surviving "in the flesh." Thus, even assuming continuity of consciousness and introspection through and beyond death, no report of such an experience could be made available for the purpose of surviving psychologists.* Psychology, as a science, limits itself to the study of embodied mind by embodied mind. This limitation Dr. Merz, clearly, assumes and accepts. And, thus, speaking of mind and body as we experience them in this life and in this world, he denies, in the technical language of philosophy, that either body or mind are "substances." Consequently, he regards the time-honoured problem of the "relation" of body and mind—especially when conceived as a relation between mind as a "spiritual" substance (a *res cogitans*) and body as a "material" substance (a *res extensa*)—as utterly misconceived. In this he is, clearly, consistent with his whole position. If by "mind" we mean the "changing firmament of thought" and by "body" a certain vivid constellation within that firmament, the term "substance" is inapplicable to either. On this point, Dr. Merz agrees with Hume:—"Neither in the shape of external matter, nor of a substantial mind, has the idea of substance any meaning or justification" (*Fragment*, p. 80). And instead of the problem of the relation of body and mind, we have only the problem of the relation between the introspective, or synoptic, point of view through which we become aware of "the totality of our conscious flow of thought or firmament of inner sight" (*ibid.*, p. 80), and the external point of view through which our attention is focussed on the selected system of sensations which stands within the conscious flow for the common outer world, and of which the body is a member. The latter point of view grows out of the former, and tends to absorb us in practical life. But for philosophical synopsis it is all-important to reverse this tendency and, by practising the introspective method, to realise the totality of which the outer world is but an abstract selection.

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* *Religion and Science*, pp. 106-7. Mr. T. Whittaker has drawn my attention to a curious parallel to the above argument in Sir Henry Taylor's *Philip van Artevelde*. Philip is relating the experiences of his philosophical youth:—

"Last came the troublesome question, What am I?
A blade, a seedling of this growth of life
Wherewith the outside of the earth is cover'd;
A comprehensive atom, all the world
In act of thought embracing; in the world
A grain scarce filling a particular place."

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* There is no evidence that Dr. Merz ever drew either Psychical Research or Spiritualism into the scope of his reflections, or that he paid any attention to the reports from beyond the grave which are alleged to come to us through mediums.

There is, however, a further aspect of the mind-body problem which is forced upon us when we reflect upon all that is involved for each of us in his perception of other persons. With this aspect we must now deal, if our account of Dr. Merz's position is to be adequate. We know already that, according to his view, the child, beginning at the level of an undifferentiated flow of experiences, advances to the recognition of a "real" world (distinguished from, though appearing in, the flow of its experiences) by means of the perception of other persons and of its own body. But, what exactly do we perceive of other persons? Nothing, so it would seem, but their bodies. Our minds are, as such, private for each of us and not directly accessible to another's observation. This is, as Dr. Merz admits, the view both of popular thought and of natural science (*Fragment*, p. 92). It is, too, the view which he adopts himself. "Of other persons, we observe and know clearly only what their bodies show: although we are firmly convinced that these bodies represent or reveal an inner life, a flow of thought, similar to our own, we know of this only through inference from and analogy with our own inner experience" (*ibid.*, p. 85). It is not altogether consistent with this that, a few pages later (p. 94), Dr. Merz throws out the fascinating, if undeveloped, suggestion of a "direct intercourse between the inner worlds" and declares that such "truly intersubjective or spiritual communion becomes more plausible if we abandon as unlikely the idea that the minds of individual persons are located and confined within their bodily framework, and are limited to communications through it." However, what does it matter if there is an inconsistency here? Such inconsistencies are, as we have reminded ourselves at the outset, to be expected in the statement of philosophical views which had, confessedly, to be left incomplete and fragmentary. We should, in the circumstances, rather admire Dr. Merz for the open-mindedness with which he accepts and emphasizes a line of thought at variance with other positions already adopted by himself. Not to commit oneself irrevocably to a view which is, perhaps, narrow and inadequate, and then to be blind to anything which might contradict that view, but to be always on the watch for fresh considerations and ready to receive and incorporate them—this, surely, is the truly synoptic temper. Hence, it would be absurd if we were to emphasize the inconsistency and, over it, forget to consider whether the suggestion here thrown out by Dr. Merz is not, perhaps, of the utmost value. To me, at any rate, it seems to be most important, and I cannot but think that Dr. Merz himself would have found the means of carrying it further, had he been acquainted with the later work of Meinong and Husserl. For, the line of thought which leads Dr. Merz to his suggestion of a direct intercourse of mind with mind starts from the part which "imitation" plays in the development of the child's mind, and, more particularly, from the way in which we understand the speech and the expressive gestures of others. If communication by language enriches and develops a mind, it is not because that mind infers, by analogy, that the use of such-and-such words must signify that the other has such-and-such thoughts, or beliefs, but because, understanding the words, it adopts their meaning as part of what it thinks and believes itself.

Seeing that Dr. Merz has given us little more than a hint, it will help us to appreciate the importance of the point if we develop it for ourselves. Let us begin with speech. Speech is a kind of bodily behaviour, perceptible by others. We hear the sounds made, we see the movements of the lips and throat. So far, then, speech fits into the formula that the body is perceptible by others, the mind is not. Indeed, we may ignore the fact that there is any mind involved. We may listen to the sounds as mere sounds, or watch the movements of the speech-organs as mere movements, just as we may listen to the rustling of leaves or watch them twinkle in a breeze. But speech is not mere sound: it is expressive sound. It is sound which reveals, conveys, communicates—what? In answer, we must distinguish. The sounds express (*ausdrücken*) the speaker's mental processes, and they mean (*meinen*, *aussagen*) a certain object or matter-of-fact.* When A remarks to B that "2+2=4" or that "Lloyd George has resigned," B may, if he chooses, infer from the words that A is so thinking and judging, *i.e.*, that these thought-processes are going on in A's mind. That would be an inference, by analogy, from perceptible behaviour to imperceptible thought; and often enough, in practical life, it is important that we should draw just such inferences from the utterances of others to the thoughts, beliefs, etc., which *are* their minds. But much more commonly when we hear the words, our own attention will be fastened not on the fact that the speaker thinks what he says, but on the meaning and the truth of what he says. We, in short, *understand* the words and accept, reject, doubt, etc., what they mean, without giving any thought whatever to the speaker's mental processes. Indeed, when a child learns to speak by imitating the speech of its elders, the marvel, as Dr. Merz points out (*l.c.*, p. 93), is how the imitative acquisition of a word brings with it some grasp of the meaning of the word, without any awareness, on the child's part, of either its own or the speaker's mental processes. This would seem to be at least part of what Dr. Merz is thinking of when he speaks of a "direct intercourse between inner worlds." The speaker's attention is concentrated on what he is thinking and saying, not on the fact of his thinking as a process in his mind; and, similarly the hearer is occupied with the meaning of the words he hears and, once again, not with either the speaker's or his own mental processes. The inner worlds meet, so to say, through their content: communication of mind with mind means that one mind, by words or other signs, stimulates another mind to perceive, think, etc., the same objects, propositions, etc., which it perceives and thinks itself. Though perceptible signs and perceptible bodily behaviour are involved, it is difficult to maintain that the hearer's understanding of the words involves an inference by analogy to the speaker's mind. In that sense, therefore, the intercourse is "direct."

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* Strictly, this statement is too simple. It fits only assertory sentences which state "judgments." It would have to be qualified for sentences conveying wishes, commands, etc. But these refinements are not necessary for our present purpose. *Cf.* my paper, *A Plea for a Phenomenology of Meaning*, in *Proceedings of the Aristotelian Society*, vol. xxi, 1920-21, and the literature there discussed.

The argument may be pushed further still. Consider the original recognition of persons. The child, surely, feels and responds to the mother's love without first going through a process of analogical reasoning to the effect that such-and-such smiles, gestures, behaviour must mean feelings of affection and joy in the mother, because similar smiles, etc., on the child's own part would mean feelings of affection in himself. The very suggestion is absurd. The theory of *Einfühlung* (" empathy "), especially in the form which Professor S. Alexander has given to it,* appears to supply a more fitting analysis of this kind of intercourse, which, once more, would clearly be an example of " direct intercourse " in Dr. Merz's sense, except that in this case the communication is one of feelings, not of facts to be believed.

In fact, the omission of an explicit theory of the way in which mind *expresses* itself through body and bodily signs constitutes a genuine lacuna in Dr. Merz's argument, which in certain passages almost cries out for completion by just such a theory. There are passages in which Dr. Merz seems to stand on the very threshold of a theory of expression, and leaves us wondering why he did not take the one step more which is needed to give unity to his whole argument. Thus, in *Religion and Science*, we read that the body, or physical self, " forms a *kind of link* between what we ordinarily consider to be entirely outside or entirely inside " (p. 130). The *Fragment* is more explicit still:—" From our point of view the body is only, and at best, a *point or region of reference*, where, in some way which we cannot picture to ourselves, the two worlds meet: a point of *transition* through which the inner self *communicates* with the outer, and *vice versa*. All analogies taken either from the inner firmament to explain the outer, or from the outer world to explain the inner, are palpably wholly useless "* (p 92) It is, surely, not far-fetched to see in phrases like "link," "point of reference," "transition," "communication of inner with outer," a tentative groping towards what we have called "expression" For, it is to be observed that, from the strictly introspective point of view, all these phrases are quite meaningless Introspectively, the body is for Dr Merz nothing but a markedly persistent cluster of sensations in the stream of consciousness Thus conceived, it is neither a "link," nor a medium of "communication" Moreover, if we apply the analogical argument on such an introspective basis, what will be the inevitable result? Nothing but this, that each of us must conceive the minds and bodies of others as introspection teaches him to conceive his own In other words, I must conceive another's mind, the existence of which I infer, as another stream of consciousness in which the other's body forms a persistent cluster of sensations, just as my body does in the stream of consciousness which is my mind But, the fatal difficulty in this account, as it stands, is precisely this, that the introspective concept of my mind as a stream of consciousness and of my body as a group of sensations in that stream supplies *no basis at all for the analogical inference* from another body to another mind. It is only in so far as I recognise my body as expressing my mind that I can try to interpret the bodily behaviour of another as expressing the mind of that other Thus, as we urged above, the concept of expression is required to bridge the gap between the introspective point of view and the analogical inference to the existence of other minds The coherence of the theory demands such a development, and Dr Merz's own tentative phrases indicate that he was conscious of the gap and sought to fill it

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* See *Mind*, N.S., Vol. xxii. (1913), No. 85, pp. 14 ff., "Collective Willing and Truth."

And, lastly, in yet another direction would such a supplementation complete the coherence of Dr Merz's views We have already noted, at an earlier point in this paper, Dr Merz's emphasis on the creativeness of mind, and his endorsement of Wundt's concept of the "growth of mental energy" and of Fouillée's concept of *idées-forces* (*cf.*, also, *Fragment*, pp. 46-9). In so far as this creativeness uses physical objects and forces for the realization of values—not leaving objects as it finds them, but remoulding them nearer to the heart's desire; and, again, in so far as this transformation is effected by the use of our bodies, as physical instruments brought to bear on a physical world, once more some such concept as "expression" seems required if this creativeness is to be made compatible with the introspective theory of the relation of body and mind. I believe that Dr. Merz, had he lived to think out his position fully, would have developed it himself in just such a direction.

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* The italics (except for *vice versa*) in these two quotations are mine

6. After all that we have said on the preceding two topics, we can afford to be short on the third topic, *viz.*, (*c*) Dr. Merz's treatment of the distinction between "self" and "not-self." For, the ground covered by the argument bearing on this distinction is almost wholly identical with the ground covered under the previous two heads.

Briefly, we may say that, for Dr. Merz, the term "self" has two meanings, an introspective and a social one. These two meanings—or the two contexts in which these two meanings are used—are not unconnected, but it is important to distinguish them because, whilst the one is fundamental and the other is derivative, yet in the course of experience the derivative meaning comes first, whereas the fundamental one is recognized only as the result of prolonged practice in introspection. The child learns to distinguish other persons long before it learns to recognise itself and to refer to itself by the personal pronoun "I." And it learns to recognize itself as a body among other similar bodies long before it is capable of the feat of introspection which reveals its own body and all other bodies as complexes of sensations in the firmament of thought. It is then, and then only, that by an act of synoptic introspection the self in its fundamental sense, as one with the whole stream of consciousness, is apprehended. Thenceforth, "the word Self acquires and retains two different meanings: the first being that small portion of the external world which is composed by our own physical frame; the second being that original, all-comprehensive stream of consciousness or firmament of the soul which contains all external as well as all internal experiences" (*Rel. and Sci.*, p. 42). Dr. Merz claims boldly that "the language of commonsense shows clearly that our self is identical with that whole of experience which constitutes at any moment our inner firmament" (*Fragment*, p. 56). His chief evidence from commonsense language is the phrase "to lose one's self," when used to describe the extreme opposite of introspective self-awareness. A person completely absorbed in the experience of a sense-object, *e.g.*, one who is "all eye and ear" at a play, has "lost himself" in that his mind is contracted to the dimensions of his experience of the exciting object, and becomes utterly forgetful of past or future, utterly unaware of the whole background of experiences which is nonetheless there all the time. But, normally, we are always more or less clearly aware of the contrast between any object which stands out, holding our attention, and the dimmer background; and when we want to mark the contrast, we oppose the outstanding object as "not-self" to the background as "self." Still, fundamentally, this is but a differentiation (as we saw before) within the one, single stream of our consciousness which, as a whole, *is* the self. Hence, there is no distinction between "experience" and the self which "has" the experience—the "experient," as we might say. On the contrary, Dr. Merz explicitly affirms the identity of "experient" and "experience," and expresses it in the formula: "Experient = Experience" (*Fragment*, p. 57). "As often as we regard our experience as a whole, and any momentary experience as a portion of this whole, we take the synoptic view and call it our Self or Ego" (*ibid.*). If such language as this about the "self" should seem paradoxical, we must remember, as Dr. Merz points out more than once, that the terms and the grammar of ordinary language are much better adapted to express our experiences of sensible objects and of their relations to each other than the findings of introspection and synopsis. With Bergson, he might have said that language was shaped under the pressure of practical and, later, of scientific interests, and thus is ill-adapted to express the insight of an attitude so unusual, and so difficult to maintain, as that of introspection. The very words by which we describe intellectual processes, *e.g.*, to "comprehend," to "understand," betray their origin in our dealings with physical and spatial facts. On the other hand, we feel that we are using language again in its ordinary sense when we give to the term "self" its derivative, social meaning. Our context, then, is a social one: each of us is a self among other selves to whom he stands in a variety of relations. Each of us is a person in a society of persons who are individually distinct from each other by reason of their separate bodies. But, of this sense of "self" Dr. Merz says little, nor is there any need for us to enlarge upon it, seeing that further discussion would only lead us back over the ground already covered in the preceding section.

We may, however, conclude this brief review of Dr. Merz's treatment of the problem of "self" with two comments.

The first is this. So long as Dr. Merz was pre-occupied with the *History of European Thought in the XIXth Cent.*, he had but little time to follow closely the new philosophical movements which have marked the opening years of the present century. And later, when after 1914 he began to put his own philosophical conclusions into shape, he found himself hampered increasingly by failing eyesight. In his last years, he was wholly dependent on the devoted help of Mrs. Merz, supplemented by the occasional services of a reader. Thus cut off from free and constant access to the literature, he was unable to familiarize himself with that form of the "neo-realistic" movement which has been developed in this country and which is especially identified with the names of Professor S. Alexander, Dr. G. E. Moore, Mr. Bertrand Russell, Professor John Laird and others. These realists agree with Dr. Merz in employing the introspective method, and in identifying "self" and "mind," yet their use of introspection leads them to results very different from those of Dr Merz, and consequently they give to "self" and "mind" a very different meaning So far from identifying ' experient " and "experience," they insist on an analysis of experience into a mental *act* (of perceiving, thinking, etc), on the one side, and an *object* on the other The experient (mind, self) is the act, or, taking a wider view, a group or "tissue" of such acts This strict and sharp distinction of act and object being of the very essence of the realistic position, it becomes, of course, impossible to construe objects like the body and the rest of the external world as elements within the self or mind The neo-realistic handling of the introspective method is, thus completely opposed to that of Dr Merz It would have been supremely interesting if we could have had from him a critical examination of neo realism, and a vindication of his own position against the realistic alternative In such a vindication Dr Merz might have called powerful witnesses to his aid For his identification of experient and experience may be paralleled from the writings of so distinguished a philosopher as F H Bradley and of so eminent a psychologist as William James

The second comment is that clearly Dr Merz's account of the self has been left in too fragmentary a state to enable us to do more than guess at the answers which he might have given to a number of important questions that come inevitably to one's mind in trying to think out his position Among these questions the most important, both for technical philosophy and for human interest is that of the relation of God to man We may note as only a minor difficulty the fact that the account which Dr Merz gives of human personality as having two aspects an inner and an outer, can hardly be transferable literally to the personality of God But a more serious problem is how we are to conceive the selfhood of God, as a person, in relation to the selves of human persons Dr Merz gives us a plurality of human selves individual, distinct streams of consciousness or firmaments of thought Human analogies, no doubt, are very inadequate for conceiving God, but, at least, we cannot think of God as *less* than such a "firmament of thought." But how, then, are the Divine mind and human minds related? We can hardly think of them as utterly self-contained and related merely by "pre-established harmony," like Leibniz's "windowless" monads. Dr. Merz's own account of our sense of the "pressure of the All" forbids such an hypothesis. But, on the other hand, he clearly had no sympathy with any one of the various theories—from the "world-soul" of the Stoics to Spinoza's all-inclusive "substance," from Schopenhauer's cosmic "will" to the *élan vital* of Bergson—which make our individual minds merely pulses, as it were, of the universal life, emanations of the universal spirit. His position, we must assume, was intermediate between the extreme pluralism of Leibniz and the extreme monism of most philosophical mystics. But what exactly it was, we cannot tell. Nay, there are good grounds for thinking that Dr. Merz took very little interest in this time-honoured problem of "the One and the Many,"* and that he had, in fact, not thought out any answer to it at all. Perhaps he would have urged that the problem is unanswerable with our limited knowledge. And it must be admitted, in fairness, that it is not a problem lying in the straight line of the enquiries into which Dr. Merz was led by his introspective method. But, if we are to push our synoptic endeavour to the bitter end, is it a problem we can avoid facing? It is at least significant that the problem has persisted down the ages and presented itself afresh to successive generations of thinkers differing widely in outlook and method. In our own day it has been debated by four of our foremost contemporary thinkers (B. Bosanquet, A. S. Pringle-Pattinson, G. F. Stout, Viscount Haldane) under the technical title "Do Finite Individuals Possess a Substantive or an Adjectival Mode of Being"? in a symposium of the Aristotelian Society.†

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* Mr. T Whittaker, whose long and intimate knowledge of Dr. Merz's philosophical thought entitles him to speak with authority on such a point as this, confirms this suggestion

† See *Proceedings*, Vol. xviii. (1917-18), and the volume, *Life and Finite Individuality*, edited for the Society by Professor H. Wildon Carr.

IV

Dr Merz s philosophical work does not lend itself to being labelled and classified He ranked himself with no school he followed no fashionable isms If we approach him with our ready made tags and try to pigeon hole him as an idealist or a realist a metaphysician or an epistemologist we have to retire baffled He does not fit neatly into any section of our tidy classifications of philosophical systems But this very fact is a tribute to his independence of mind In all his thinking he sought only to find the truth and to express it as he found it *Jurare in verba magistri* was as foreign to his nature as was wilful striving after paradox and the cheap originality which is only eccentricity Masters indeed he acknowledged but what he learned from them he made his own and where he found them wanting he did not hesitate to supplement and to correct First and last he stood on his own feet drawing his very strength as a thinker from the selfless humility which waits upon the truth to reveal itself to a mind kept always wide open to all the lessons of many sided experience

> Then sight or that which to the soul is sight
> As by a lightning flash will come to thee "

These lines from the *Dream of Gerontius* which he prefixed as one of two mottos to *Religion and Science* express the spirit of his work as much as the Platonic ο μὲν γαρ συνοπτικὸς διαλεκτικος ὁ δε μη οὔ which strikes the key note of the *History* Withal he was a lonely thinker, cut off by the claims of business and industry from regular contact with the academic fraternities of philosophers among whom otherwise he might have looked for and found companionship and the encouragement of friendly criticism He is the more to be honoured and admired for the work which single handed he achieved *

Comparing Dr Merz with his contemporaries among philosophers it is easy to see that the bent of his mind was not metaphysical But neither was he interested in the technicalities which delight experts in the theory of knowledge If we say that he was chiefly interested in the Philosophy of the Human Mind, we shall get nearest *to the truth* Moreover, this term has the merit *of* reminding us of the debt which Dr Merz owed, on the one hand, to Lotze's *Mikrokosmus*,* and, on the other, to the British School of Philosophy † With the latter school, he refused to discriminate between psychology and philosophy Adopting from it the combination of introspective and genetic methods he adopted also the principle that the study of the way in which concepts are formed and beliefs come to be adopted, at the same time throws light on their validity and justification But, it is to be noted that he applies this principle with much more confidence in the truth of our concepts and beliefs than is characteristic of his English forerunners He does not repeat Locke's critical proposal 'to examine our own abilities, and see what objects our understandings are or are not fitted to deal with ' Still less does he let the argument lead him to Hume's sceptical conclusions No, in his confidence in the human mind, *i e*, in the essential trustworthiness of human thought, he is the true child of the Kantian and Post Kantian philosophy When he accepts religion as a fact and merely asks, "How is religion possible?" we must clearly understand that he accepts religion as "spiritual *knowledge*," *i e*, he accepts the religious view of the world as substantially *true* He follows Hume's method of tracing every "idea" to an "impression" which is its "original" in the modified form of tracing our scientific and religious systems of thought to their basis in primitive consciousness But he corrects Hume's analytic habit of mind by pointing to the *ensemble* of experiences and the need of synopsis, and he corrects Hume's sceptical conclusion that our beliefs are psychologically necessary but not rationally justifiable, by the essentially Kantian and Post-Kantian confidence that science and religion are rational, for all that they are also psychological "growths" in the human mind. And to call them "rational" and "knowledge" is, I repeat, to accept them as trustworthy, *i.e.*, as revealing the real nature of the world we live in. The truth of science, as we have seen, is for Dr. Merz relative, being subject to the qualification that science is "abstract." The truth of religion is for him, clearly, absolute. Or, at least, if it is subject to qualifications, these qualifications spring, not from abstraction, but from the inherent finiteness of the human mind. At any rate, religion is not an illusion; it is not even, as some would have it, a wholesome, life-preserving illusion: something which, though false, is good for man to believe. No, for Dr. Merz, religion reveals the fundamental nature of reality as completely as our minds can grasp it.

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* In this connection however the scholarly and competent help which Mr. T Whittaker gave Dr Merz in seeing the volumes of the *History* and the later writings through the press deserves to be gratefully commemorated.

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* This work, rather than Lotze s *Metaphysik* influenced Dr Merz's own philosophical programme (*Cf* *Fragment*, p ix)

† *Cf* Such sentences as those from the "Introduction " to Hume s *Treatise on Human Nature* " 'Tis evident, that all the sciences have a relation, greater or less, to human nature Even *Mathematics*, *Natural Philosophy*, and *Natural Religion*, are in some measure dependent on the Science of MAN In pretending therefore to explain the principles of human nature, we in effect propose a compleat system of the sciences, built on a foundation almost entirely new, and the only one upon which they can stand with any security "

In other words, the foundations of Dr. Merz's non-sceptical estimate of human thought are ethical and religious. In this he is the disciple rather of Kant and Lotze than of Locke and Hume. There can be little doubt that if Dr. Merz had developed more fully that non-introspective strain in his thought which leads him to dwell on the human mind as creative, as the source of all values in the world, as expressing itself in all the achievements of civilisation, he would have been compelled to emphasise the ethical and religious foundations of his confidence much more explicitly than he actually does in his published writings. As it is, we are conscious of a certain gap between the conclusions yielded by his philosophical enquiries and the convictions which both sustained his practical life and, as it were, beckoned to him from afar on his philosophical pilgrimage. Perhaps the full spirit of the man and the thinker appears nowhere better than in the closing sentences of his course of lectures on "the Study of Mental Philosophy," delivered to the Literary and Philosophical Society, at Newcastle, in 1882. Taken down, at the time, by one of his hearers, they ran as follows:—"To feel enthusiasm for the great, but to be faithful in the little—in this is the enjoyment of seeing and realizing the living personal Spirit of God, in a world of personal spirits which He has created. The view of this world, this Universe, is wonderful, true poetry, but we must not take the name of this wonder recklessly, but faithfully and diligently cultivate the smaller sciences—and in time Philosophy will lead us to the possession of the Promised Land."

Did he reach the promised land of his philosophical hopes? In one sense he always possessed it. In another sense, he was always reaching out for it. His life, though long and full of work, yet proved too short for the task which he had set himself. There is a saying of Sir Thomas More's which I found copied out among Dr. Merz's papers. It may have heartened him in some moment of discouragement at having to leave his work unfinished, and so he noted it down:—"Better it were insufficiently done than utterly undone" This, surely, says all that need be said, when the work of a man who has given of his best is hampered by blindness and cut short by death. But, when that work is the work of a philosophical thinker, there is yet a further consideration which we may fitly give in Dr. Merz's own words. "The ultimate reality is not to be reached by thought, but must be felt, lived, and experienced; and when human language and human ideas fail, creation in art and living events must come to our aid."* Dr. Merz lived his philosophy even more completely than he put it on paper, and we shall misconceive the full expression of it, unless we think, not merely of his writings, but of his happy family-life, of the success of his industrial work, and, above all, of the spirit of sweet and wise humanity which endeared him to all who were privileged to count themselves among his friends.

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* *History*, Vol iv., p. 769.

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THE VELOCITY OF OXIDATION OF NITRIC OXIDE AND ITS IMPORTANCE IN NITROGEN FIXATION

By George W Todd, M A , D Sc , F Inst P

[Read Dec 14th, 1922]

Introduction

The rate at which nitric oxide is converted into the peroxide under varying conditions of concentration and temperature is of the utmost importance in nitrogen fixation and nitric acid manufacture

In the arc processes nitric oxide is synthesised at high temperatures and subsequently oxidised at lower temperatures In the ammonia oxidation process nitric oxide is formed at about 500°C, the temperature depending on the catalyst, and oxidised at a much lower temperature by the addition of " secondary " air In the Hauser process the nitric oxide is produced by exploding a mixture of illuminating gas and air, with or without the addition of oxygen, the resulting products being cooled rapidly by sudden expansion to prevent the dissociation of the nitric oxide

The nitric oxide obtained in these processes is usually oxidised to peroxide by the addition of air and the peroxide absorbed in water towers Among other things, the reactions in the towers depend on the temperature and concentration The most important reaction is of course, the oxidation of the nitric oxide Since this reaction, at the concentrations met with in practice, is a slow one, time and space must be allowed for it whatever may be the treatment of the resulting peroxide

Below 140°C, nitrogen peroxide does not appreciably dissociate back to nitric oxide and oxygen, so that below this temperature the oxidation of nitric oxide may be regarded as a complete termolecular reaction, as the experiments of Lunge and Berl* show.

The velocity of a complete termolecular gas reaction.—The velocity of the complete reaction $2NO + O_2 \rightarrow 2NO_2$ is given by $\frac{dC}{dt} = k\, C^2_{NO}.C_{O_2}$

where C = concentration, and k = constant for a given temperature

Let us suppose that at the beginning of the reaction there are $2a$ molecules of NO and b molecules of O_2 in a reaction space v. Let the number of molecules of NO oxidised in time t be $2x$, then the velocity of the reaction at this instant will be given by

$$\frac{d}{dt}\left(\frac{x}{v}\right) = k\left(\frac{a-x}{v}\right)^2\left(\frac{b-x}{v}\right)$$

N.B.—$(a-x)/v$ is only half the usually accepted concentration of NO. This merely alters the constant k.

Let the *oxygen* be in *excess*, *i.e.*, $b > a$.

The velocity equation may be written

$$\frac{d}{dt}\left(\frac{x}{v}\right) = k\left(\frac{a}{v}\right)^3\left(1-\frac{x}{a}\right)^2\left(\frac{b}{a}-\frac{x}{a}\right)$$

When the reaction takes place *at constant volume* this becomes

$$\frac{dx}{dt} = k\frac{a^3}{v^2}\left(1-\frac{x}{a}\right)^2\left(\frac{b}{a}-\frac{x}{a}\right)$$

Putting $\frac{x}{a} = X$ where X is the fraction of NO converted,

$\frac{b}{a} = p$, and $k\left(\frac{a}{v}\right)^2 = K$, we get

$$\frac{dX}{dt} = K(1-X)^2(p-X) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If the *nitric oxide* is *in excess*, the velocity equation becomes

$$\frac{dX}{dt} = K_1(p_1-X)^2(1-X) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $\quad K_1 = k\left(\frac{b}{v}\right)^2$ and $p_1 = \frac{a}{b}$

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* Z. angew. Ch. 18, 807, 1906
,, ,, ,, 20, 1713, 1907.

When the reaction takes place *at constant pressure* we can no longer regard v as a constant unless the concentration of NO or of O_2 is very small

Let the *oxygen* be *in excess* *i e* $b > a$

The velocity equation

$$\frac{d}{dt}\left(\frac{x}{v}\right) = k\left(\frac{a}{v}\right)^3\left(1 - \frac{x}{a}\right)^2\left(\frac{b}{a} - \frac{x}{a}\right)$$

becomes

$$\frac{1}{v}\frac{dx}{dt} - \frac{x}{v^2}\frac{dv}{dt} = k\left(\frac{a}{v}\right)^3\left(1 - \frac{x}{a}\right)^2\left(\frac{b}{a} - \frac{x}{a}\right)$$

If the pressure is maintained constant throughout the reaction then the volume at any time will be proportional to the number of molecules present The initial volume when $t = o$ is therefore

$$v_o \propto 3a + (b - a)$$

where $(b - a)$ is the excess of oxygen present The volume of this excess remains the same throughout the reaction while the volume of the $3a$ molecules changes continuously with time The volume after a time t is

$$v \propto 3a(1 + \alpha X) + (b - a)$$

where α is a constant which is negative in the reaction we are considering and $X = \frac{x}{a}$ = the fraction of NO converted

Whence we obtain

$$v = v_o\frac{3a(1 + \alpha X) + (b - a)}{3a + (b - a)}$$

and $$\frac{dv}{dt} = \frac{3a\alpha v_o}{3a + (b - a)}\frac{dX}{dt}$$

On substituting these values for v and $\frac{dv}{dt}$ in the velocity equation and simplifying we get

$$\frac{dX}{dt} = k\left(\frac{a}{v_o}\right)^2\frac{(1 - X)^2\left(\frac{b}{a} - X\right)}{1 + \frac{3a\alpha X}{2a + b}}$$

or putting $\frac{b}{a} = p$,

$$\frac{dX}{dt} = k\left(\frac{a}{v_o}\right)^2\frac{(1 - X)^2(p - X)}{1 + \frac{3\alpha}{2 + p}X} \qquad (3)$$

When there is no change in volume, $\alpha = 0$, and the equation reduces to (1) If in addition to the $2a$ molecules of NO and the b molecules of O_2 there are also initially present $n \cdot a$ molecules of an inert gas, then

$$v = v_0 \frac{3a(1 + \alpha X) + (b - a) + na}{3a + (b - a) + na}$$

so that
$$\frac{dX}{dt} = k\left(\frac{a}{v_0}\right)^2 \frac{(1 - X)^2 (p - X)}{1 + \dfrac{3\alpha}{2 + p + n} X} \quad . \quad . \quad . \quad . \quad . \ (3a)$$

If the *nitric oxide* is *in excess*, *i.e.*, $a > b$, we have by similar reasoning, since

$$v_0 \propto 3b + 2(a - b)$$

$$\text{and } v \propto 3b \cdot (1 + \alpha X) + 2(a - b)$$

$$\frac{dX}{dt} = k\left(\frac{b}{v_0}\right)^2 \frac{(p_1 - X)^2 (1 - X)}{1 + \dfrac{3\alpha}{1 + 2p_1} X} \quad . \quad . \quad . \quad . \quad . \ (4)$$

where $p_1 = \frac{a}{b}$.

If $n \cdot b$ molecules of an inactive gas are also present in the initial mixture, it is easy to show that equation (4) becomes

$$\frac{dX}{dt} = k\left(\frac{b}{v_0}\right)^2 \frac{(p_1 - X)^2 (1 - X)}{1 + \dfrac{3\alpha}{1 + 2p_1 + n} X} \quad . \quad . \quad . \ (4a)$$

These equations can be integrated and from the solutions, general curves, showing the percentage conversion of NO to NO_2 with time, can be plotted both for the case when the reaction takes place at constant volume and for the case when the reaction takes place at constant pressure. For the solutions of the equations and the general curves for any complete homogeneous gas reaction between two reactants, the reader should refer to previous work.*

The experiments of Lunge and Berl.—Lunge and Berl (Z. angew. Ch. 20,1713, 1907) made experiments on the velocity of oxidation of nitric oxide at constant pressure and at a temperature of 20°C.

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* Todd. *Phil. Mag.*, xxxv., 281 and 435.

Their observations are given in fig 1.

Let us compare the theoretical curves with those obtained experimentally by Lunge and Berl. At 20°C a considerable proportion of the peroxide will be in the form N_2O_4. We will, however, assume first that the reaction is

$$2NO + O_2 \rightarrow 2NO_2 \qquad i.e., \ a = -\frac{1}{3}$$

Lunge and Berl's curve I., fig. 1, corresponds to

$$p = \frac{500}{62 \cdot 5} = 8.$$

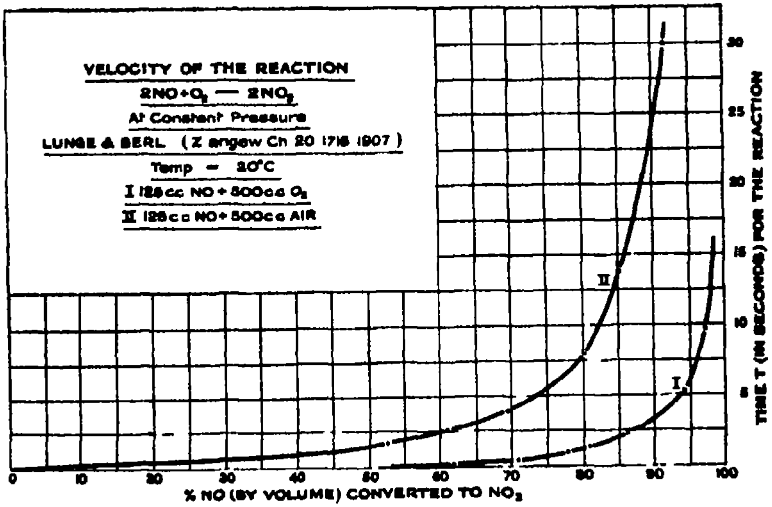

FIG. 1.

On comparing the curve $p = 8$ (see page 439, *Phil. Mag.*, XXXV.), with curve I., fig. 1, we get:—

% NO converted (= 100X).	t sec Lunge and Berl.	$k\left(\frac{a}{v_0}\right)^2 t$ Theoretical curves.	$\therefore k\left(\frac{a}{v_0}\right)^2$
75	1	·375	·375
85	2	·75	·375
90	3	1·15	·383
94	5	1·95	·390

The agreement in the last column is very good

Now let us assume that the reaction is

$$2NO + O_2 \rightarrow N_2O_4 \qquad i.e \quad a = -\frac{2}{3}$$

On comparing the curve $p = 8$ (see p 441 *Phil Mag* xxxv) with curve 1 fig 1 we get —

NO converted (100X)	t sec Lunge and Berl	$k\left(\frac{a}{v_0}\right)t$ Theoretical curves	$k\left(\frac{a}{v_0}\right)^2$
75	1	370	370
85	2	67	335
90	3	1 0	360
94	5	1 70	340

The agreement in the last column is again good

Lunge and Berl's curve II fig 1 corresponds to

$$p = \frac{100}{62\ 5} = 1\ 6$$

A graph for this value of p has not been plotted in the papers referred to The case is one in which molecules of an inactive gas (nitrogen) are present We must use the equation (3a) viz

$$\frac{dX}{dt} = k\left(\frac{a}{v_0}\right)^2 \frac{(1 - X)^2\,(p - X)}{1 + \frac{3a}{2 + p + n}X}$$

In the case we are considering $n = \frac{400}{62\ 5} = 6\ 4$ Hence taking $a = -\frac{1}{3}$ we have

$$\frac{dX}{dt} = k\left(\frac{a}{v_0}\right)^2 \frac{(1 - X)^2\,(1\ 6 - X)}{1 - \frac{X}{10}}$$

$$\therefore k\left(\frac{a}{v_o}\right)^2 t = \int_0^X \frac{\left(1 - \frac{X}{10}\right) dX}{(1 - X)^2(1{\cdot}6 - X)}$$

$$= \frac{7}{3} \log_e \frac{1{\cdot}6(1 - X)}{1{\cdot}6 - X} + \frac{3}{2} \cdot \frac{X}{1 - X}.$$

Putting in a few values of X gives :—

X	·7	·8	·85	9
$k\left(\frac{a}{v_o}\right)^2 t$	1·88	3·86	5·84	10·06

Comparing these figures with Lunge and Berl's curve II., we obtain

% NO converted (= 100X).	t sec. Lunge and Berl	$k\left(\frac{a}{v_o}\right)^2 t$ Theoretical.	$\therefore k\left(\frac{a}{v_o}\right)^2$
70	4·0	1·88	·470
80	7·6	3·86	·507
85	13·5	5·84	·432
90	24·0	10·06	·419

Taking $a = -\frac{2}{3}$, we have

$$\frac{dX}{dt} = k\left(\frac{a}{v_o}\right)^2 \frac{(1 - X)^2 \cdot (1{\cdot}6 - X)}{1 - \frac{X}{5}}$$

$$\therefore k\left(\frac{a}{v_o}\right)^2 t = \int_0^X \frac{\left(1 - \frac{X}{5}\right) dX}{(1 - X)^2(1{\cdot}6 - X)} = \frac{17}{9} \log_e \frac{1{\cdot}6(1 - X)}{1{\cdot}6 - X} + \frac{4}{3} \cdot \frac{X}{1 - X}$$

Putting in a few values for X gives :—

X	·7	·8	·85	·9
$k\left(\frac{a}{v_o}\right)^2 t$	1·83	3·60	5·38	9·21

Comparing with Curve II Fig 1 we obtain

% NO converted (= 100X)	t sec Lunge & Berl	$k\left(\frac{a}{v_0}\right)^2 t$ Theoretical	$k\left(\frac{a}{v_0}\right)^2$
70	4.0	1.83	457
80	7.6	3.60	473
85	13.5	5.38	398
90	24.0	9.21	384

The value of the velocity constant at 20°C.—We can now determine the velocity constant k in the equation

$$\frac{d}{dt}\left(\frac{x}{v}\right) = k\left(\frac{a-x}{v}\right)^2\left(\frac{b-x}{v}\right)$$

where a is the number of gm mols of NO initially present in v litres of reaction space and b the number of gm mols O_2 initially present in v litres of reaction space.

The average values of $k\left(\frac{a}{v}\right)^2$ obtained from Lunge and Berl's experiments are

Curve I		Curve II	
$\alpha = -\frac{1}{3}$	$\alpha = -\frac{2}{3}$	$\alpha = -\frac{1}{3}$	$\alpha = -\frac{2}{3}$
381	349	456	428

The numbers from Curve I and Curve II show considerable variation. We must remember, however, that α really lies between $-\frac{1}{3}$ and $-\frac{2}{3}$. Since in Curve II, the concentration of peroxide formed in a given time is lower than in Curve I owing to the diluent nitrogen the ratio of NO_2 to N_2O_4 will be greater, and therefore α will be nearer to $-\frac{1}{3}$ than in Curve I.

The time taken in mixing the gases might easily be of the order of a second, so that values of $k\left(\frac{a}{v_0}\right)^2$ obtained for small t's might be considerably in error We will therefore take for the average value of $k\left(\frac{a}{v_0}\right)^2$, the last figures in each table, for which the reaction times are large The figures are 390, 340, 419, 384, giving for an average

$$k\left(\frac{a}{v_0}\right)^2 = 383$$

The value of $\left(\frac{a}{v_0}\right)$ was the same for Curves I and II In each experiment 62.5 cc NO (½ total) in 625 cc was the initial concentration Since 24,000 cc at 20°C = 1 gram molecule of gas

$$\frac{a}{v_0} = \frac{62.5}{24000} \times \frac{1000}{625} \qquad \text{gm mols per litre}$$

$$\text{Whence } k = \frac{383}{\left(\frac{1}{240}\right)^2} = 22000$$

Knowing the initial concentration of nitric oxide and the ratio of oxygen to ½ nitric oxide, we can now obtain from the general curves, the time for any percentage conversion of nitric oxide to nitric peroxide at a temperature of 20°C

Application to the Ammonia-Oxidation Process—In processes for the oxidation of ammonia, air and ammonia in the proportions required by the equation

$$4\,NH_3 + 5\,O_2 = 4\,NO + 6\,H_2O$$

are passed over the catalyst at a temperature of about 500°C The resulting products are quickly cooled since nitric oxide is unstable at 500°C and the water is taken out "Secondary" air is then introduced into the mixture of nitrogen and nitric oxide for the oxidation of the latter It is advisable that this oxidation should take place in an empty tower The time of contact required for any percentage conversion can be calculated from the general curves Since a large volume of inert gas is present the change in volume at constant pressure will be insignificant. Hence the curves for a termolecular reaction at constant volume will be used.*

Take an example. Suppose that in an actual plant for Ammonia Oxidation, the nitric oxide present immediately before entering the oxidation tower is 4 volumes in 28 volumes. Air is introduced for the oxidation.

For $p=1$, *i.e.*, just sufficient oxygen for complete oxidation, 10 volumes of air must be introduced, making the total volume 38.

Initial concentration of NO is 2 vols. in 38.

$$\therefore \frac{a}{v} = \frac{2}{24000} \div \frac{38}{1000} = \cdot 0022 \text{ gm mols. per litre at } 20°\text{C}.$$

$$\therefore k\left(\frac{a}{v}\right)^2 = 22000 \times (\cdot 0022)^2 = 0{\cdot}106$$

From the curves, (loc. cit.) $k\left(\frac{a}{v}\right)^2 t = 50$ for 90 per cent. conversion.

$$\therefore \quad t = \frac{50}{\cdot 106} = 472 \text{ sec.} = 7{\cdot}9 \text{ minutes.}$$

For $p = 2$ *i.e.*, 20 vols. of air added to 28 vols. of mixture.

Initial concentration of NO is 2 vols. in 48 vols.

$$\therefore \frac{a}{v} = \frac{2}{24000} \div \frac{48}{1000} = \cdot 00174$$

$$\therefore \quad k\left(\frac{a}{v}\right)^2 = 22000 \times (\cdot 00174)^2 = 0\cdot 067$$

From the curves $k\left(\frac{a}{v}\right)^2 t = 7{\cdot}5$ for 90 per cent. conversion.

$$\therefore \quad t = \frac{7\cdot 5}{\cdot 067} = 112 \text{ sec.} = 1{\cdot}9 \text{ min.}$$

$$k\left(\frac{a}{v}\right)^2 t \text{ for 95 per cent. conversion} = 17$$

$$\therefore \quad t = \frac{17}{\cdot 067} = 255 \text{ sec.} = 4{\cdot}3 \text{ minutes.}$$

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[*] See *Phil. Mag.*, xxxv., plate ix., fig. 2.

Proceeding in this way with various values of p, we obtain the following table:—

TABLE A.

Initial content of $NO = 4$ vols. in 28 vols. Air added for oxidation.

For 90 per cent. conversion:—

$\frac{\text{Vol. of air added}}{\text{Original vol. of mixture}}$	$\frac{10}{28} = \cdot36$	$\frac{20}{28} = \cdot71$	$\frac{30}{28} = 1\cdot1$	$\frac{40}{28} = 1\cdot4$	$\frac{50}{28} = 1\cdot8$
Time (min.)	7·9	1·9	1·5	1·3	1·5

For 95 per cent. conversion:—

$\frac{\text{Vol. of air added}}{\text{Orig. vol. of mixture}}$	$\frac{20}{28} = \cdot71$	$\frac{30}{28} = 1\cdot1$	$\frac{40}{28} = 1\cdot4$	$\frac{50}{28} = 1\cdot8$
Time (min.)	4·3	3·4	3·1	3·3

It will be observed that there is a *minimum contact time* for a definite percentage conversion. For the particular case we are considering it is impossible to get 90 per cent. conversion with a contact time of less than 1·3 minutes and it is impossible to get 95 per cent. conversion with a contact time of less than 3·1 minutes. The ratio of added air to original volume is the same in each case for the minimum conversion time. See fig. 2.

If a high conversion is required with a shorter contact time in the oxidation tower, *pure oxygen* must be introduced instead of air. This will not only increase the rate of oxidation but will also result in a gas much more concentrated with respect to nitrogen peroxide.

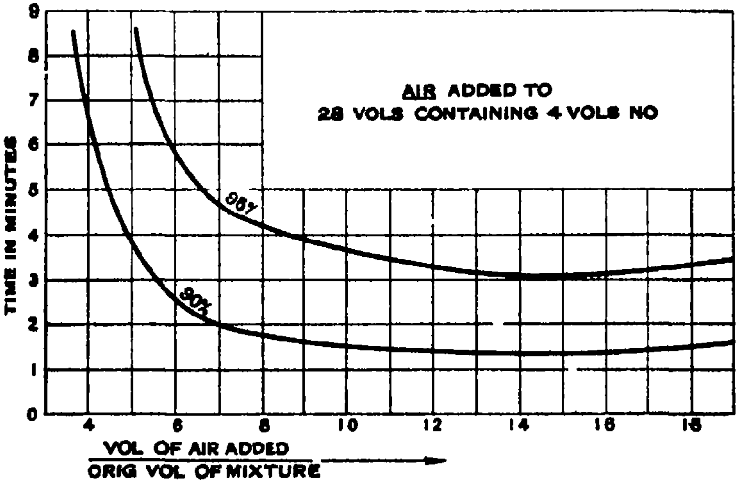
Let us work out the times for conversion when pure oxygen instead of air is introduced.

For $p = 1$, *i.e.*, just sufficient oxygen for complete oxidation, we must add two vols. to 28 of the mixture making the total volume 30.

Initial concentration of NO is 2 vols in 30 vols

$$\frac{a}{v} = \frac{2}{24000} \div \frac{30}{1000} = \cdot 0028$$

$$k\left(\frac{a}{v}\right)^2 = 22000 \times (\cdot 0028)^2 = 0\cdot 169$$



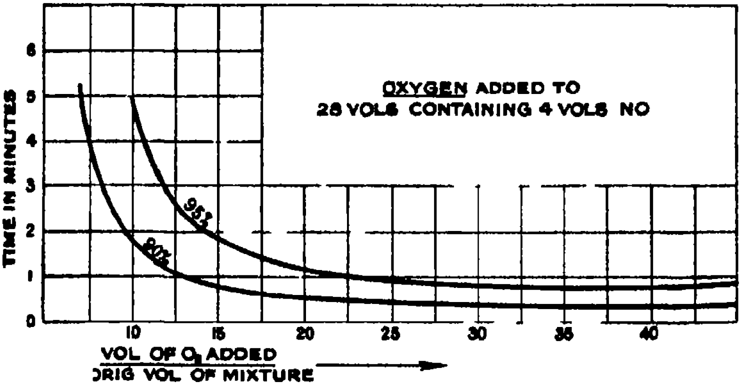


Fig 2

From the curves (loc cit)

$$k\left(\frac{a}{v}\right)^2 t = 50 \text{ for 90 per cent conversion}$$

$$t = \frac{50}{\cdot 169} = 296 \text{ secs} = 4\cdot 9 \text{ minutes}$$

For $p=2$, *i.e.*, 4 vols. of oxygen added to 28 vols. of mixture

$$\frac{a}{v} = \frac{2}{24000} \div \frac{32}{1000} = \cdot 0026$$

$$\therefore \quad k\left(\frac{a}{v}\right)^2 = 22000 \times (\cdot 0026)^2 = \cdot 148$$

$$k\left(\frac{a}{v}\right)^2 t = 7\cdot 5 \text{ for 90 per cent. conversion.}$$

$$\therefore \quad t = \frac{7\cdot 5}{\cdot 148} = 51 \text{ sec } = 0\cdot 85 \text{ min.}$$

$$k\left(\frac{a}{v}\right)^2 t = 17 \text{ for 95 per cent conversion.}$$

$$\therefore \quad t = \frac{17}{\cdot 148} = 115 \text{ sec } = 1\cdot 9 \text{ min.}$$

Proceeding in the same way gives us the following table:—

TABLE B.

Initial content of NO -4 vols. in 28 vols. *Pure Oxygen* added for oxidation.

For 90 per cent. conversion.—

Vol of oxygen added / Orig. vol of mixture	$\frac{2}{28}$ = 07	$\frac{4}{28}$ = 14	$\frac{6}{28}$ = 21	$\frac{8}{28}$ = 28	$\frac{10}{28}$ = 36
Time (min.)	4·9	0 85	0·5	0 4	0 3

For 95 per cent. conversion:—

Vol of oxygen added / Orig vol. of mixture	$\frac{4}{28}$ = ·14	$\frac{6}{28}$ = 21	$\frac{8}{28}$ = ·28	$\frac{10}{28}$ = 36
Time (min.)	1·9	1 0	0 9	0 8

The reaction times for high percentage conversions are shortened very considerably when pure oxygen is added for the oxidation.

The tables A and B are given graphically in fig. 2.

A general expression for the time of oxidation of nitric oxide produced in the ammonia oxidation process.

Since the nitric oxide content of the gas leaving the converter and condensing plant varies with the plant used, it might be useful to deduce a general formula for the time of conversion of any percentage of nitric oxide when (1) percentage content of NO (2) percentage conversion required (3) ratio of added (*i.e.*, secondary) air to original volume of gas, are known.

The general equation for the reaction at constant volume* is

$$k\left(\frac{a}{v}\right)^2 t = \frac{1}{(p-1)^2}\left\{\frac{X(p-1)}{1-X} + \log_e \frac{p(1-X)}{p-X}\right\}$$

Now $$p = \frac{\text{volume of } O_2}{\frac{1}{2}\text{volume of NO}} = \frac{\frac{1}{5}\text{(volume of added air)}}{\frac{1}{200}(\%\text{NO})\text{(volume of orig. gas)}}$$

$$= \frac{40\text{ (volume of added air)}}{(\%\text{NO in orig. gas})\text{ (vol. of orig. gas)}}$$

Also

$$\frac{a}{v} = \frac{\frac{1}{200}(\%\text{ NO})\text{ (vol. of orig. gas)}}{\left\{\text{vol. of added air} + \text{vol. of orig. gas}\right\}} \times \frac{1}{24}\text{ gm mols. per litre}$$

Also percentage conversion = 100X

Putting N = % NO in original gas

C = % conversion of total NO required

$$R = \frac{\text{volume of "secondary" air added}}{\text{volume of original gas}}$$

we obtain the following expression for the time in seconds when the temperature is 20°C:—

$$t = \frac{1045\,(R+1)^2}{(40R-N)^2}\left\{\frac{C(40R-N)}{N(100-C)} + \log_e \frac{40R(100-C)}{4000R-CN}\right\}$$

If oxygen is introduced instead of the "secondary" air and if R is now the ratio of added oxygen to the original gas the expression becomes:—

---

* This will be near enough if the per cent. NO in the gas from the converters and condensing arrangements is not greater than 15 per cent.

$$t = \frac{1045(R+1)^2}{(200R - N)^2}\left\{\frac{C(200R - N)}{N(100 - C)} + \log_e \frac{200R(100 - C)}{20000R - CN}\right\}$$

By differentiating these equations with respect to R and putting the result equal to zero, the ratio R (of added air or oxygen to original gas) which will give the minimum time of oxidation will be obtained

Application to Arc Processes.—In the arc processes the gases from the arc contain only low percentages of nitric oxide, the reaction may therefore be regarded as taking place at constant volume.

As an example let us assume that the gas coming from the arc is air containing 2 per cent. nitric oxide. The value of $p=20$. The curves (loc. cit.) do not go to $p=20$, but we proceed thus:—

$$k\left(\frac{a}{v}\right)^2 t = \frac{1}{(p-1)^2}\left\{\log_e \frac{p(1-X)}{p-X} + \frac{X(p-1)}{1-X}\right\}$$

$$\frac{a}{v} = \frac{1}{24000} + \frac{100}{1000} = \cdot 00042$$

$$\therefore k\left(\frac{a}{v}\right)^2 = 22000 \times (\cdot 00042)^2 = \cdot 00387$$

For 50 per cent. conversion, *i.e.*, $X = \cdot 5$. $k\left(\frac{a}{v}\right)^2 t = \cdot 0502$,

$\therefore t = 13 \cdot 0$ sec.

For 90 per cent. conversion, *i.e.*, $X = \cdot 9$, $k\left(\frac{a}{v}\right)^2 t = \cdot 495$,

$\therefore t = 128$ sec.

It is easy to deduce a general expression for the time required to oxidise any percentage of the total nitric oxide in arc gases.

Let the gas be *air* containing P per cent of nitric oxide. Then

$$\frac{a}{v} = \frac{\frac{P}{2}}{24000} \div \frac{100}{1000} \text{ gm. mols. per litre}$$

$$\therefore k\left(\frac{a}{v}\right)^2 = 22000 \times \left(\frac{P}{4800}\right)^2$$

We have also

$$p = \frac{\text{percentage of oxygen}}{\frac{1}{2}\text{ percentage of nitric oxide}} = \frac{\frac{1}{5}(100 - P)}{\frac{P}{2}}$$

On substituting these values in the general equation we get

$$t = \frac{2{\cdot}62 \times 10^4}{(200 - 7P)^2}\left\{\frac{X(200-7P)}{5P(1 - X)} + \log_e \frac{(200 - 2P)(1 - X)}{200 - 2P - 5X}\right\}$$

where t is the time *in seconds* required for the oxidation of a fraction X of the nitric oxide present in an arc process gas consisting of air containing P per cent. of nitric oxide, when the temperature is 20°C.

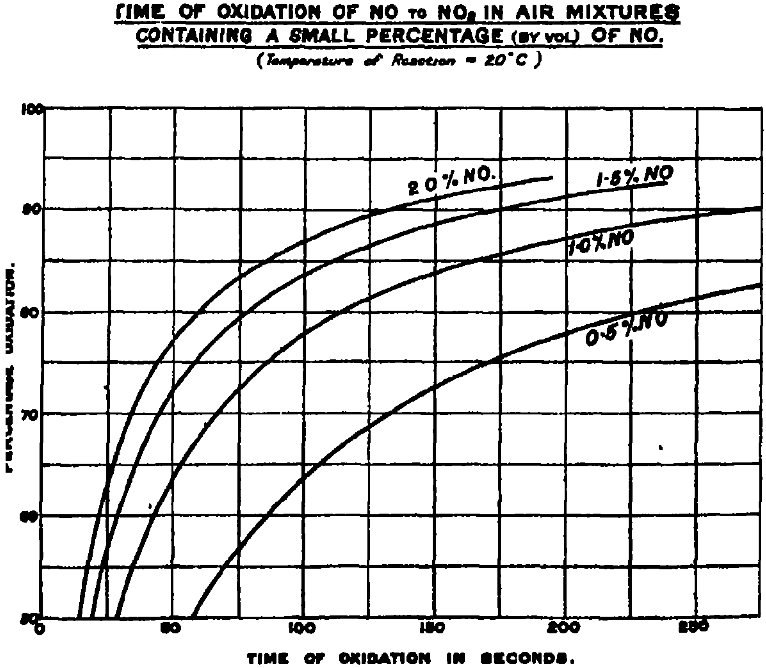

TIME OF OXIDATION OF NO TO NO_2 IN AIR MIXTURES CONTAINING A SMALL PERCENTAGE (BY VOL) OF NO.
(Temperature of Reaction = 20°C)

Fig. 3.

The following table and the curves in fig. 3, have been obtained from the above expression, TABLE.

X	P = ·5%	P = 1·0%	P = 1·5%	P = 2·0%
	t sec.	t sec	t sec.	t sec.
·5	52·9	26·7	17·9	13 5
·6	79·5	40·0	26 9	20 4
·7	124	62·4	41 5	32 0
·8	213	108	72 3	55·1
·9	480	250	164	125

Example:—Find the time required for 80 per cent. conversion of the NO in the cooled arc gases in which the percentage of NO is 1·5 per cent. In this case X = ·8, whence t = 72·3 seconds.

Concluding remarks.—The general curves which have been worked out theoretically by the writer, hold for any complete reaction at any temperature. The value of k for the temperature at which it is desired the curves should apply must be determined experimentally.

The value of k at 20°C for the oxidation of nitric oxide has been obtained from the experiments of Lunge and Berl. Bodenstein (Z. angew. Chem. 1153, 1909) and others have made experiments confirming those of Lunge and Berl. His experiments, and those of Foster and Blick (Z. angew. Ch. 2018, 1910) indicate that k changes very little with temperature. In fact they find a small negative coefficient. Hence the general expressions which have been worked out in this paper for the Ammonia Oxidation and Arc processes, using the value of k at 20°C will not be very far from the truth at neighbouring temperatures.

A SIMPLE DERIVATION OF VAN DER WAAL'S VAPOUR PRESSURE EQUATION WITH A NOTE ON MOLECULAR DIAMETERS.

By S. P. OWEN, M Sc., A.Inst.P.

[Read December 14th, 1922.]

§1.

Considering the change of a liquid into its vapour as a unimolecular heterogeneous reaction, it is easy to deduce Van der Waal's Vapour Pressure Equation from the kinetic theory.

The forces acting on a molecule in the liquid, are, on the average, directed towards the liquid along the normal to the surface, hence the assumption is made that only those molecules possessing a certain critical velocity normal to the surface, are capable of escaping from the liquid.

The number of molecules per unit volume which have velocity components between u and $u + du$, v and $v + dv$, w and $w + dw$ is

$$N\left(\frac{km}{\pi}\right)^{\frac{3}{2}} e^{-km(u^2 + v^2 + w^2)}\,du\,dv\,dw$$

where $k = \frac{1}{2R\theta}$ and N is the number of molecules per unit volume.

The number which hits unit area of the surface per second

$$= N\left(\frac{km}{\pi}\right)^{\frac{3}{2}} e^{\ km(u^2 + v^2 + w^2)} u\,.\,du\,.\,dv\,dw$$

where u is the velocity normal to the surface. Of these, only molecules having a velocity greater than the critical velocity u_1, escape into the vapour, and there number is—

$$n_1 = N_1\left(\frac{km}{\pi}\right)^{\frac{3}{2}} \int_{\infty}^{\infty} \int_{-\infty}^{\infty} \int_{u_1}^{\infty} e^{-km(u^2 + v^2 + w^2)} u\,du\,dv\,dw$$

$$= \frac{N_1}{2} \cdot \left(\frac{1}{\pi km}\right)^{\frac{1}{2}} e^{-km u_1^2}$$

Since all the vapour molecules hitting the surface pass into the liquid

$$\therefore n_l = \frac{N_v}{2} \cdot \left(\frac{1}{\pi km}\right)^{\frac{1}{2}}$$

where the suffixes l and v denote the liquid and vapour respectively.

Consider the reaction

$$\text{liquid} \rightleftarrows \text{vapour.}$$

For equilibrium conditions $\eta_v = n_l$ and $k_v N_v = k_l N_l$ where k denotes the velocity constant. Therefore the equilibrium constant

$$K = \frac{k_l}{k_v} = \frac{N_v}{N_l}$$

Since the vapour pressure is independent of the quantity of liquid present, K is proportional to p, the vapour pressure.

$$\therefore p = A\frac{N_v}{N} = Ae^{-kmu_l^2}$$

where A is a constant.

At the critical temperature, the critical energy $\frac{1}{2}mu_l^2$ vanishes, since the two phases are now identical, hence we assume that $\frac{1}{2}mv_l^2$ is proportional to $(\theta_c - \theta)$ where θ_c is the critical temperature on the absolute scale.

$$\text{Thus } p = Ae^{-\frac{B}{R\theta}(\theta_c - \theta)}$$

where B is a constant.

$$\text{When } \theta = \theta_c \qquad p_c = A \cdot$$

$$\therefore \quad p = p_c e^{-\frac{B}{R}\frac{(\theta_c - \theta)}{\theta}}$$

$$\therefore \quad \log\frac{p_c}{p} = \frac{B}{R}\,\frac{(\theta_c - \theta)}{\theta \cdot}$$

which is Van der Waal's Equation since B/R is a constant.

Since the critical energy $\frac{1}{2}mu_l^2 = B(\theta_c - \theta)$, it follows that the constant $B\theta_c$ for any substance represents the critical energy necessary for a molecule to escape at absolute zero, It is interesting to note that if $\frac{1}{2}mu_1^2$ is assumed constant and is associated with the heat absorbed in any homogeneous gaseous or heterogeneous reaction, it follows that

$$\log \mathrm{K} = -\frac{1}{2}\frac{mu_1^2}{\mathrm{R}\theta}.$$

$$\therefore \quad \frac{d}{d\theta}\log \mathrm{K} = \frac{1}{2}\frac{mu_1^2}{\mathrm{R}\theta^2}.$$

a result which is identical with that derived on thermodynamical grounds by Van't Hoff. Thus the above reasoning may be applied to homogeneous gaseous and heterogeneous reactions, by associating chemical activity only with those molecules which have a certain definite kinetic energy.

§2.

The kinetic energy necessary for a molecule to penetrate the surface of its liquid completely, is used up in doing work against the forces of molecular attraction. It is reasonable to suppose that when the molecule escapes, it has associated with it, a certain amount of potential energy which is equal to this work, the potential energy being manifested at the surface of separation in the form of Surface Tension.

A somewhat similar view has been suggested by Taylor "On the coalescence of liquid spheres," Phil Mag., Vol. 41, 1921. It is the appearance of this paper that has led the author to communicate the above point of view, which he has held since 1917, but which he refrained from publishing, owing to the difficulties involved in the conception.

It follows from the view outlined above that—

$$\begin{aligned} \pi d^2\tau &= \text{critical kinetic energy} \\ &= \mathrm{B}(\theta_c - \theta) \end{aligned}$$

where d is the diameter of a molecule and τ the surface tension. It has been shown by Eötvos (*Annalen der Physik*, 27, 448, 1885) that $\tau(\mathrm{M}v)^{\frac{2}{3}} = k(\theta_c - \theta)$

where M is the molecular weight, v the specific volume and k a constant.

Therefore $$d = \sqrt{\frac{\mathrm{B}\,(\mathrm{M}v)^{\frac{2}{3}}}{\pi\, k}}$$

Writing Van der Waal's Equation in the form

$$\log_{10}\frac{p_c}{p} = f\,\frac{(\theta_c - \theta)}{\theta}.$$

$$f = \frac{B}{R}\log_{10} e.$$

$$\therefore B = \frac{fR}{\cdot 4343}$$

$$\therefore d = \sqrt{\frac{f.\,R\,(Mv)^{\frac{1}{3}}.}{\pi k \times \cdot 4343.}} \quad . \quad . \quad . \quad . \quad (1)$$

Using the values of f given in Lewis, Physical Chemistry, Vol. I., p 123-4, and $k = 2{\cdot}12$, the value given by Ramsay and Shields, the following values of d are obtained:—

			Unit 10^{-8} cm.				
			Molecular diam. d deduced from				
	Density	f	(1)	η	k	b	D
Oxygen .. .	1·2*	2·5	3·23	3·39	3·11		2·79
Nitrogen . .	·8*	2·27	3·4	3·5	3·31	3·53	2·97
Carbon Dioxide .	·7*	2·86	4·59	4·18	4·32	3·4	4·42
Argon ...	1·4*	2·25	3·13	3·36		2·86	4·43
Water ...	1	3·26	3·23	4·09			3·45
Ether ...	·718	3·01	5·56				7·6*
Ethyl Alcohol ..	·79	3·91	5·24				5·2*

η = viscosity
k = thermal conductivity
b = Van der Waal's constant
D = limiting density.
* Landolt—Börnstein Tabellen.
Other results Kaye and Laby.

Thus the results obtained from (1) agree very closely with those obtained by the usual methods.

A NEW METHOD OF DETERMINING RADIATING AND IONIZING POTENTIALS NOT NECESSITATING LOW PRESSURES.

By W. J. CLARK, B.Sc.

[Read March 12th, 1923.]

If the molecules of a gas are subject to bombardment by an electron stream, any encounters taking place do not involve internal molecular absorption of energy unless these electrons possess an amount of energy equal to or greater than "$V_R e$," "e" being the electronic charge, and "V_R" the first Radiating Potential of the gas. In this latter case, such encounters result in energy transference sufficient to bring about displacement to less stable orbits, of valency electrons of the molecular systems affected, and hence diminution in velocity of the bombarding particles.

An investigation, therefore, of the changes occurring both in the velocities of the electrons constituting a current passing through a gas, and of the value of this current under suitable conditions, should afford experimental evidence of the existence of Radiating Potentials.

The following apparatus, similar in form though not in principle to that usually employed for the purpose, appears to be capable of supplying this evidence, advantages claimed for it being:—

(1) The pressure at which experiments are conducted need only be of the order of 1 mm.; consequently the magnitude of disturbing effects due to the presence of impurities, or to mercury vapour, is reduced, and the necessity of keeping a constant stream of gas at low pressure circulating through the experimental tube, obviated.

(2) The instrument used for detecting changes in the electronic current at and above the Radiating

Potential need not be nearly so delicate as for those methods in which such detection depends on the measurement of the photo electric effect

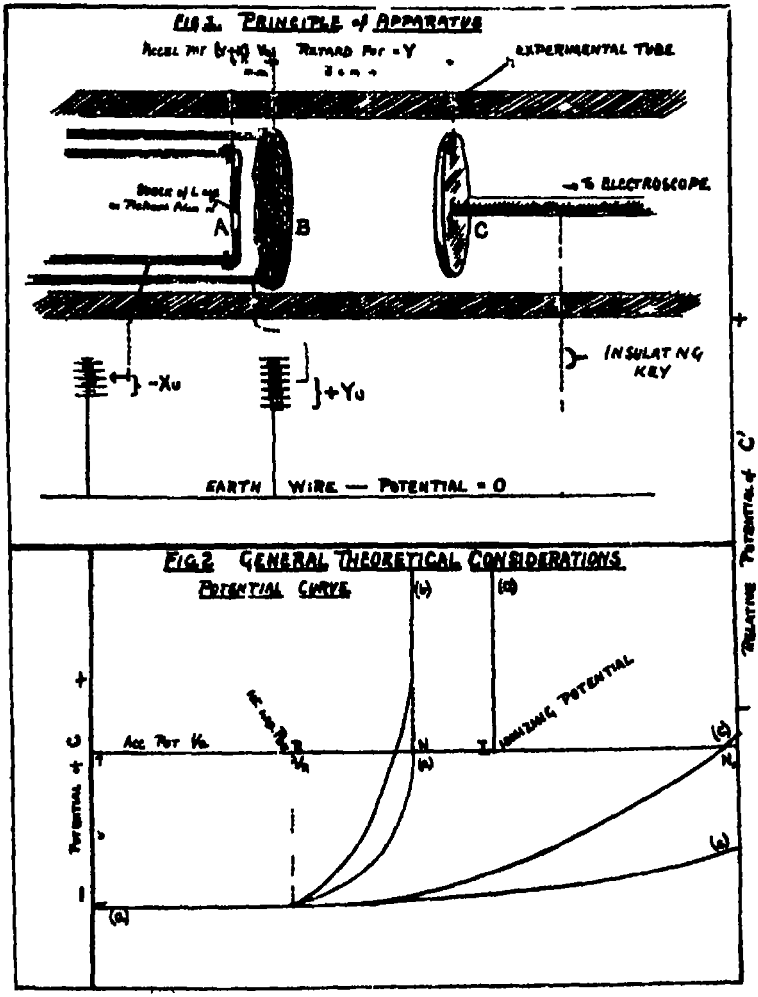


Fig 1. Principle of Apparatus

Fig 2 General Theoretical Considerations

Potential Curve

produced by radiation from stimulated molecules falling on the collecting plate of the electroscope

An experimental tube is set up containing a lime-coated platinum filament " A " as electron source, a disc-shaped gauze " B," and a circular copper plate " C," connected to an electroscope. (*Vide* Fig. 1.) " X " and " Y " are sources of variable potential difference, the accelerating potential " *Va* " applied to electrons emitted from " A " being equal to (Y + X)*v*. Between " B " and " C," a retarding potential " Y," diminishes the velocity of those electrons possessing at " B " the maximum value " *Va*," to " X " volts minus that due to the velocity lost in traversing the gas-filled space " AC '—this latter factor incidentally necessitates, in accurate determinations of " *Va*," a correction which may amount to as much as 0·5 volts for that proportion of the velocity lost between " A " and " B." (For the sake of convenience, velocities will be referred to in terms of potentials.)

General Theoretical Considerations.

Giving " X " some suitable constant small value, sufficient when " *Va* " is less than the Radiating Potential to charge " C " to a potential " *p*," it is evident that variation in " Y," while increasing " *Va* " (always subject to the condition that " *Va* " is less than the Radiating Potential) and also increasing the thermionic emission from " A," will leave the oncoming electrons with the same residual velocity corresponding to " *v* " on reaching " C."

Suppose now " *Va* " to equal " V_R," the Radiating Potential. For various reasons, electrons possessing all velocities from " O " to " *Va* " will pass through the interstices of gauze " B." The fastest electrons corresponding to " *Va* " will therefore have just sufficient energy to produce radiation effects."

Further, let the gas pressure be such that all electrons eligible for energy transference are subject to effective collisions.

As a result, they will no longer be able to traverse the space " BC " against the retarding potential " Y "; hence both the negative current at " C," and the potential to which " C " is raised will decrease.

Increasing Va" to $(V_R + x)$ results in all electrons possessing velocities between "Va" and $(Va - x)$ being stopped with further diminution of the negative current and value of v" Due to this cause alone, when Va" equals $(V_R + X)$, the negative charge received by C" will decrease to zero and "v' will become "0" Finally, when "Va' is equal to the Ionizing Potential "V," encounters will now result in complete separation of electrons from the parent molecules, positive ions being formed and a positive current being registered by the electroscope, which will charge up to a potential of the order of "V Fig 2 shows this effect graphically, ordinates being the potential to which "C is raised, and abscissæ the corresponding value of "Va" Several other superimposed effects, however, very considerably modify the above curve

(1) Radiation from stimulated atoms causes emission of photo-electrons from "C" This effect, negligible at the Radiating Potential itself, compared with the loss of charge of "C due to the actual stoppage of an electron will increase progressively as the stoppage becomes more general and "N" the neutral point of the above curve, will move considerably nearer to "R" in consequence

(2) Cumulative radiation effects due to the absorption of radiant energy by already stimulated molecules will result in premature ionisation occurring, and hence further decrease in the value of "N," in fact the combined action of (1) and (2) may lead to a merging of the two parts of (*a*) into a curve similar to (*b*), "C" charging up slowly beyond the limits of measurement as soon as all the original current has been stopped

If the pressure of the gas is, however, not such that all electrons are stopped, when "Va" is equal to the Radiating Potential, a number of the latter will still reach "C," charging it up though more slowly to its former value "v," and not until "Va" has attained a sufficiently high value to make the range in which these electrons are still eligible big enough to ensure encounters for all or till sufficient ionization has taken place to counteract the negative charge received by " C," will " v ' decrease in value Hence " R," the point at which

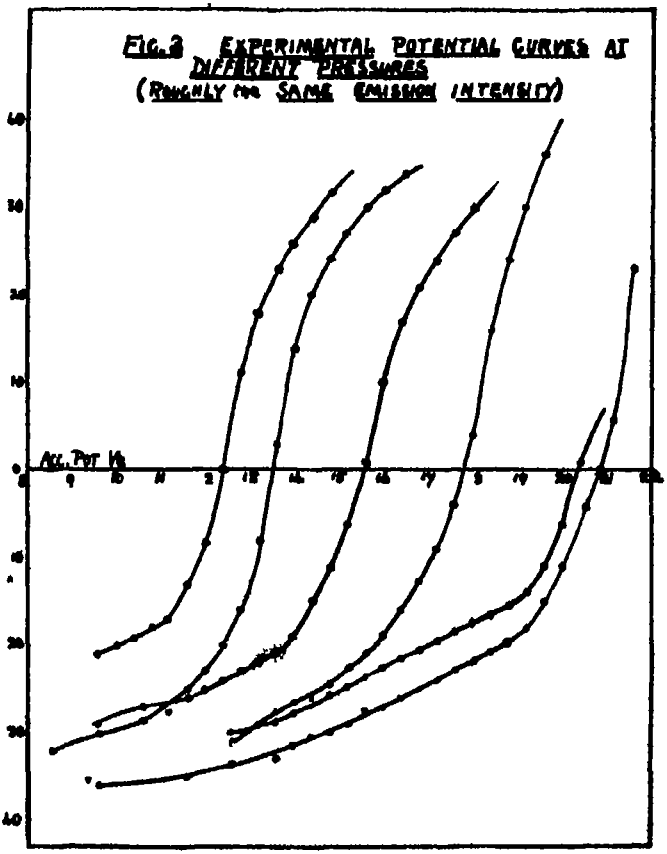


FIG. 3 EXPERIMENTAL POTENTIAL CURVES AT DIFFERENT PRESSURES (ROUGHLY THE SAME EMISSION INTENSITY)

this decrease becomes perceptible will assume a greater value than " V ", " N " also will correspond to a higher value of " Va,' indicating a condition in which the positive and negative charges received by " C " just neutralize each other, at low pressures it will disappear altogether (*vide* Fig 2c)

But if by this means the Radiating Potential is masked, a measurement of the current received by "C," ought to indicate by its diminution an absorption of energy at the Radiating Potential and independent except for its magnitude on the pressure

Evidently the efficacy of the above methods depends on a knowledge of suitable conditions in order to bring out those characteristics of the electronic current capable of definite interpretation

Preliminary Experiments

The following experiments were therefore conducted as preliminaries in ascertaining these conditions air being used as most convenient and capable of indicating qualitatively the possibilities of the method "X was fixed at 5 6 v and a number of readings at different pressures with roughly similar emission intensity from "A" taken Fig (3) illustrates the results obtained —

(*a*) An inspection bears out the expectation of a change in the position of the neutral point with alteration in pressure this position varying from 12 5 v for a pressure of 1 mm to 21 v for 0 17 mm

(*b*) At higher pressures the rate of decrease of the negative potential to which "C' is raised is considerably greater than at the lower ones, necessarily suggesting less photo electric action in the latter cases

(*c*) It is impossible to detect the position of the first Radiating Potential *i e* the potential corresponding to initial decrease of "*v*' All the curves show a slight decrease even in the vicinity of 10 v but due partly to the fact that the electroscope was least sensitive in registering the highest values of the potential to which "C" was raised and also due to the difficulty experienced in determining accurately these highest values, since the final stage of the charging up of "C' was brought about only by the comparatively few electrons possessing the maximum velocity " *Va* " at " B," no definite indication of an initial decrease is possible. Nevertheless the general trend of all the curves suggests that the Radiating Potential must be below 9·5 v.

(*d*) None of the curves indicate the pressure to be such that all electrons have been stopped as soon as their energy exceeds that corresponding to the Radiating Potential, for if this were the case, " C " would charge up positively to an extent far exceeding that indicated, that is to a value comparable with " Y "; all the positive values of " *v* " must apparently be potentials of equilibrium between the residual part of the original negative current and the photo-electric and ionization effects combined.

. . . .

Keeping " X " the same value (5·6 v.), a further set of experiments was now conducted to investigate the effect of variation in the intensity of emission of the electron stream. Fig. 4 gives an idea of the general effects obtained at two different gas pressures. They are quite representative of similar curves obtained for intermediate pressures. It appears that:—

(1) increase in emission increases the value of " *Va* " corresponding to " N,"

(2) the magnitude of the increase depends on the pressure, being greatest for the lowest pressure.

Considered theoretically one would have expected a greater proportion of ionization for an intense emission, and hence a decreased instead of an increased value for " N." The most feasible explanation of (1) is that increase in ionization does take place but the ions formed are unable because of their small mobility to charge up " C," to an extent proportional to their rate of production. This small mobility may be due to the combining together of numbers of positively charged particles to form aggregates. The electrons constituting the original current, having greater

Fig. 4. Effect of Varying Emission Intensity at Different Pressures — 1mm and ·24mm.

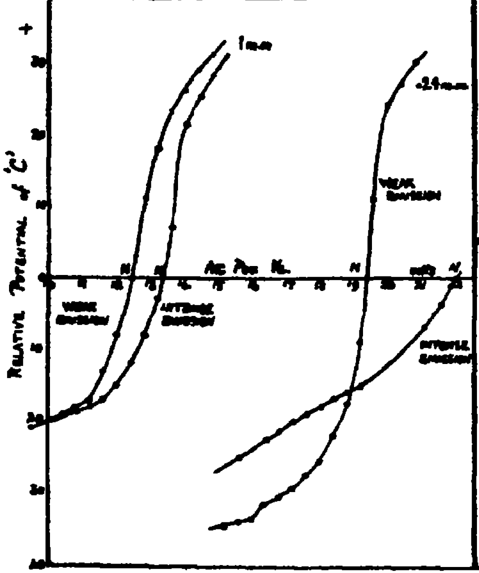


Fig. 5. Current Curves at Rod Pot. Theoretical Considerations

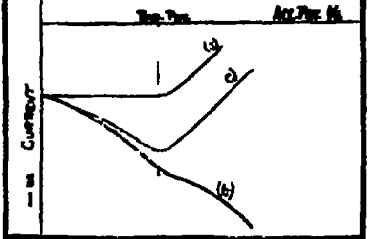


Fig. 6 Experimental Results

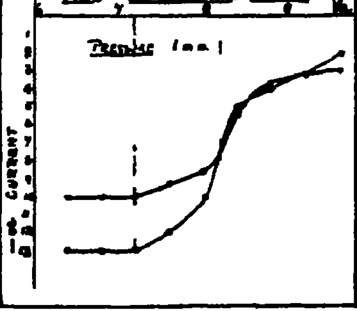
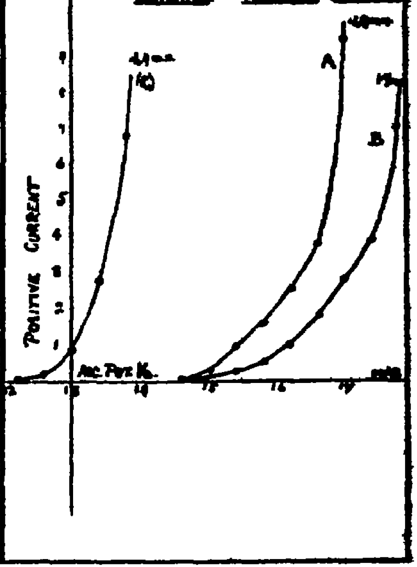


Fig. 7 Current Curves Indicating Ionization Thresholds [illegible]



mobility on account of their smaller bulk, are not so impeded, hence for intense emission, a greater proportion of negative than positive ions can reach " C," than for weak emission.

(2) Might be explained as follows. At higher pressures, the number of electrons reaching " C " at the neutral point " N " is less, and the degree of ionization greater, than at low pressures, hence variation in emission intensity means current variation at " C " of less magnitude for high than for low pressures, consequently a smaller variation in " *Va* " will suffice for compensation.

EXPERIMENTS ON RADIATING POTENTIAL.

From a consideration of these preliminary experiments it appears that in order to obtain decisive initial effects corresponding to the Radiating Potential, it will be better to investigate changes in current value at this point. Turning to Fig. 5, curves (*a*), (*b*) and (*c*) represent theoretically, three possible relations between " V*a* " and the negative current reaching " C."

If change in " V*a* " has very little effect on the number of electrons drawn from " A," then the negative current (i) reaching " C," will be of constant value until " V*a* " is equal to the Radiating Potential, when a decrease of magnitude depending on the gas pressure will result, as illustrated by curve (*a*).

If on the other hand, increasing " V*a* " increases the emission from " A," (i) will increase in value till the Radiating Potential is reached. At low pressures, the number of electrons subject to effective collisions and hence prevented from reaching " C," will be less than the increased number drawn from " A " as " V*a* " is increased, so that (i) will still increase in value, though not so rapidly as before (see *b*). If, however, the pressure is such that most of the electrons eligible for energy transmission are stopped, there will be a considerable decrease in the negative current as illustrated in curve (*c*), at the Radiating Potential.

Several preliminary experiments with X at different values indicated the suitability of 5 6 v again At low pressures increasing V_a only resulted in a slight increase in the negative current similar to that indicated in Fig 5*b* at pressures above 1 5 mm no negative current at all could be detected but for pressures of the order of 1 mm curves of the type indicated in Fig 6 were obtained

Apparently increase in V_a did not affect the emission from A appreciably the current being constant until V_a equalled 7 2 v at this point a decided current decrease was observable followed by a further decrease at 8 2 v These value of V_a are uncorrected for the retarding effect of the gas molecules between A and B on the electron stream which at the above pressure was estimated to amount to 0 5 v Subtracting this correction from the above there are two distinct current decreases one corresponding to 6 7 v and a further larger one to 7 7 v These values are only qualitative but correspond roughly to the Radiating Potentials of Oxygen and Nitrogen respectively

Further experiments conducted with apparatus only slightly modified showed that very slight changes in the construction of the latter made very considerable modifications necessary with regard to the adjustments of pressure filament emission and the value of X in order to emphasize the current characteristics illustrated above

Experiments on the Ionizing Potential

An attempt was now made to eliminate the effects most likely to mask the true Ionising Potential These effects are due to —

(1) photo electric action and premature ionisation and

(2) the negative current which persists if the gas pressure is not such that all electrons gaining energy corresponding to V_a are subject to effective collisions

To eliminate (1) as small an emission as possible was used—this incidentally meant a corresponding decrease in the number of positive ions produced at the Ionising Potential.

To decrease (2), " X " was changed to 1·4 v., so that only electrons having velocities between " Va " and (Va–1·4) v. at " B," could reach " C " below the Radiating Potential.

Fig. 7 illustrates the results obtained, the emission from " A " being adjusted so that no negative current could be detected below the point of emergence of the positive current from the horizontal axis. Ionization is indicated as occurring between 14 5 v. and 15 v., corresponding to the Ionising Potential of Oxygen. The most decisive results were obtained between 0 5 mm. and 1 mm. pressure. Decreasing the pressure, decreased to such an extent the amount of ionization produced, while at the same time making it necessary to diminish still further the emission from " A " in order to prevent a negative current reaching " C," that measurement of this ionization was rendered exceedingly difficult and uncertain.

Curve (c) for a pressure of 0 49 mm. is interesting, as showing that when the value of the negative current reaching " C " has been made very small, increasing the emission, results in premature ionization occurring, thus considerably lowering the value of " Va " corresponding to " N "; this effect is just the opposite to that illustrated in connection with the preliminary set of potential curves of Fig. 4, where the increased ionization produced is more than counterbalanced by the corresponding increase in the negative current reaching " C."

. . . .

The author expresses his indebtedness to the Department of Scientific and Industrial Research for the maintenance grant which enabled him to carry out these experiments and to Professor G. W. Todd for his advice during the period of the research.

SOME FURTHER PROPERTIES OF CELLULOID WITH A DISCUSSION ON THE BASIC PRINCIPLES OF OPTICAL METHODS OF MEASURING STRESS AND STRAIN.

By H. E. LANCE MARTIN, B.Sc., Assoc.M.Inst.C.E.

[Read Oct. 31st, 1922.]

I.—INTRODUCTION.

The description of the experiments presented herewith are a further contribution to the problem of the optical determination of stress and strain in the transparent substance "CELLULOID." Whilst the results must not be taken as final, the Author submits that they will help to explain the phenomena observed when strained celluloid is examined by means of polarised light.

From 1911 to 1914 some preliminary experiments were made[1] with the object of testing the distribution of stress in celluloid, reinforced with mild steel wire. The experience thus gained showed that the method of measurement then adopted, although sound, was very difficult to control and was thus open to improvement. With this end in view the Author decided to re-investigate the whole subject, and work was commenced in April, 1922, continuing, at intervals, to the present time.

The material used was in no wise treated, either to remove the initial stress, or to improve its optical properties. In the course of time some of the specimens after being worked, that is loaded and unloaded, altered, otherwise the specimens were made from the material as received from Messrs. The British Xylonite Co., who informed the Author that it was of recent manufacture.

Similar work is now being carried out with material which has been kept for over nine years. This possesses excellent optical properties. The comparison of the results obtained from the old and the new materials will be exceedingly valuable and instructive when available.

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[1] 1.—Some experiments with Reinforced Materials examined by aid of Plane Polarised Light. Liverpool Engineering Society *Transactions*, Vol. XXXVI.

II.—APPARATUS.

In the earlier experiments the polarised light was obtained by the reflection of a parallel beam of light from a piece of black polished glass, while the analyser was a small nicol prism.

In the present experiments the polariser and analyser consist of a large pair of nicol prisms suitably mounted. The source of light is a 1,500 C.P. "Focuslight," which lamp gives a steady illumination and possesses the great advantage of requiring no attention after once having been adjusted.

The nicol prisms, lamp, condensers, and lenses are all mounted on an optical bench which is divided into two portions by a large gap in which are placed two straining or testing machines. Both of these are of the single lever type, giving a maximum load on the specimen of approximately 200 lbs. At all loads they are sensitive to plus or minus 1 lb., and one of them is so constructed that the specimen can be turned through an angle of 90 degrees in its own plane. This specimen is generally a simple standard for comparison with the specimen under examination.

By a simple arrangement of the lenses a real image of the standard can be made to fall on the specimen, with the result that the objective lens can throw properly focussed images of both specimen and standard on to the viewing screen. This screen can be replaced either by a camera for taking colour records, or by the slit of a Hilger's fixed-arm spectrometer, by means of the wave-length drum attachment of which, wave lengths, and hence retardations, can be measured to 10 Angstroms units or 1 $\mu\mu$. It is possible to estimate to one-tenth of this unit, but it was found from readings taken by two independent observers that the mean error of one reading, from one set of 7 observations of the same wave length, was 2·24, and that the greatest mean error was 4 7 However if the readings were taken by one observer these errors were about halved Hence any observation will be taken as being within plus or minus 2 $\mu\mu$ This figure will naturally be increased for all orders of colours (retardations) other than unity

For obtaining the strains in the celluloid specimens a Ewing s extensometer was used This instrument can measure to one five-thousandth part of an inch while estimates to one-tenth of this are possible

III —MECHANICAL PROPERTIES

The determination of Young s Modulus of Elasticity was undertaken Many experiments have been made previously[2] but apparently without time effects being noted It will be shown that the Modulus depends on a number of factors and is by no means constant The same specimen was used throughout and was made from celluloid cut from a large sheet Its section measured 1 inch by 0 170 inch and its length was such that the standard distance of 8 inches between the gauge screws on the extensometer could be easily accommodated In no single case was the so-called elastic limit approached as it was desired to examine the behaviour of the material under

TABLE I —EXPERIMENT APRIL 7TH 1922

Stress in lbs per sq inch	Extensometer Readings.	Time in Minutes from commencement of Experiment	Remarks.
151 8	0 60	0	Modulus from curve 386 500 lbs sq in
199 4	1·01	1	
247 0	1 55	2	Unloading At 151 8 lbs sq in 6 minutes from commencement Ex Reading—0 72
294 5	2 11	3	
342 3	2 54	4	

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[2] —The stress strain properties of nitro cellulose and the law of its optical behaviour By Prof. E. G Coker F R.S. and K. C. Chakko, M.Sc. Phil. Trans Series A Vol. 221

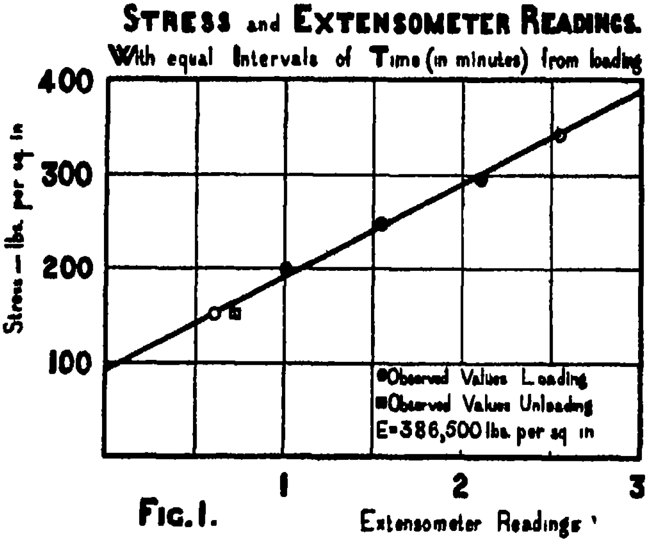

STRESS and EXTENSOMETER READINGS.
With equal Intervals of Time (in minutes) from loading
400
300
200
100
Stress — lbs. per sq. in
● Observed Values Loading
■ Observed Values Unloading
E = 386,500 lbs. per sq. in
1
2
3
Extensometer Readings

FIG. I.

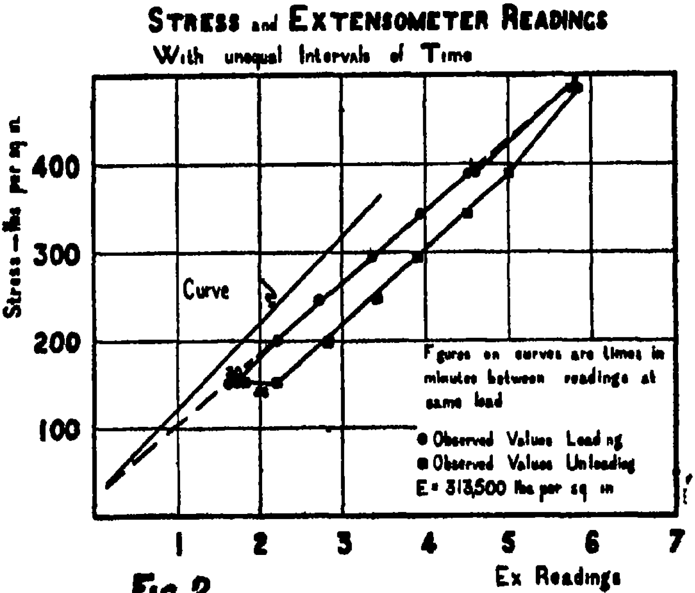

STRESS and EXTENSOMETER READINGS
With unequal Intervals of Time
400
300
200
100
Stress — lbs per sq in.
Curve
Figures on curves are times in minutes between readings at same load
● Observed Values Loading
■ Observed Values Unloading
E = 313,500 lbs per sq in
1
2
3
4
5
6
7
Ex Readings

FIG. 2

very low stresses Table I gives the stresses time of loading and extensometer readings and Fig 1 shows the resulting curve as obtained by the least square method The value 386 500 lbs per sq in for Young s Modulus was calculated from this line and had the extensometer readings been taken as soon as practicable after each load instead of allowing one minute to elapse in each case this value would have been slightly higher

TABLE II —EXPERIMENT APRIL 10TH 1922

Stress lbs sq inch	Extensometer Readings	Time in Minutes	Remarks
151 8	1 61	0	Loading
151 8	1 71	30	
199 4	2 20	35	
247 0	2 72	(35 5)	
294 5	3 35	36	
342 3	3 95	38	
389 9	4 52	40	
389 9	4 62	41	
485 2	5 75	42	
485 2	5 85	44	
389 9	5 00	45	
342 3	4 50		Unloading
294 5	3 91	47 5	
247·0	3 40		
199 4	2 81		
151 8	2 20	48 5	
151 8	1 81	90 5	

The effect of allowing the loads to remain constant for some considerable time is shown in Fig 2 which was plotted from the data given in Table II The extensometer reading of 1 61 was taken as soon as practicable after the application of the first load which caused a stress of 151 8 lbs per sq inch This stress was maintained for a period of 30 minutes in which time the reading had increased to 1 71 The three constant stress periods for loading are indicated in the figure by horizontal steps on which the periods of rest are shown The material revealed similar characteristics when being unloaded Fifty-eight minutes after the stress had been reduced to 151 8 lbs per sq inch

TABLE III – EXPERIMENT, APRIL 11TH, 1922.

Stress lbs sq in	Ex. Readings Loading	Total time in Minutes	Total time from application of new load	Ex. Readings Unloading	Total time	Total time from removal of each load.
(No load)	0·31	0	0	0 22	(18 hours)	(18 hours)
				0 69	38 5	3·0
				0 71	37·5	2·0
				0 73	36·5	1·0
				0·78	36·0	0·5
				0·95	35·5	0 0
151·8	1·82	0 0	0	2·78	35·5	3 0
	1·98	1 0	1	2·80	34·5	2·0
				2 83	33·5	1·0
	2 00	2 0	2	2 85	33 0	0·5
				2 90	32 5	0·0
247·0	2·99	2 0	0	4·00	32 5	3·0
	3·02	2 5	0·5	4·00	31·5	2·0
	3·05	3·0	1·0	4·08	30·5	1·0
	3·15	3·5	1·5	4 08	30·0	0·5
	3 15	5·0	3 0	4·12	29 5	0
342 3	4·26	5·0	0	5·21	29·5	4·0
	4·31	5 5	0·5	5·21	28·5	3·0
	4·34	6·0	1·0	5·23	27·5	2·0
	4·35	6·5	1·5	5·25	26·5	1·0
	4 36	7 0	2·0	5·25	26·0	0·5
	4·37	7 5	2·5	5·31	25·5	0
	4·38	8 0	3 0			
509·0	6·42	8 0	0	7·25	25·5	5·5
	6 55	8 5	0·5	7 25	24·5	4·5
	6·57	9·0	1·0	7 25	23 5	3·5
	6·58	9 5	1 5	7 25	23·0	3·0
	6·61	10 0	2·0	7·27	22·5	2·5
	6·62	10 5	2 5	7 27	22 0	2·0
	6 70	11 0	3 0	7·31	21·5	1·5
	6 71	11 5	3·5	7 32	20·5	0·5
	6 71	12·0	4 0	7 40	20·0	0
675·0	8·70	12·0	0			
	8·81	12·5	0·5	9·05	20·0	
	8·82	13·0	1·0			
	8·91	13·5	1·5			
	8·91	14 0	2·0			
	8·92	14·5	2·5			
	8·93	15 0	3·0			
	9·05	20 0	8·0			

the readings dropped from 2 20 to 1 81 Complete recovery required 1 71 This would probably have been reached had more time been allowed It is also probable that if the specimen had been loaded and unloaded with very long intervals between the successive stresses the extension and the recovery curves would have been close to if not identical with each other The Modulus value obtained from the line drawn evenly amongst the loading points (neglecting the three steps) is 313 500 lbs per sq inch or nearly 19 per cent lower than the first value obtained two days previously

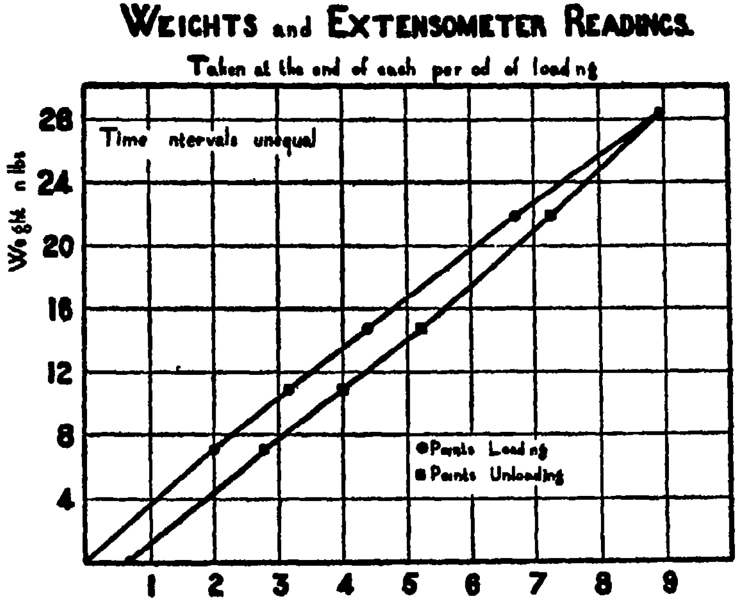


FIG. 3.

Time and previous loading having such a marked effect on the material the following experiment was framed to investigate these important characteristics in greater detail

At definite times the specimen was loaded extensometer readings were taken (*a*) as soon as practicable after loading and (*b*) at stated intervals after loading Similar observations were made when unloading Table III gives the

TABLE IV

		Extensometer Readings						Calculated Stress				Modulus				
	Time Intervals	0	1	2	3	4	5	0	1	2	3	0	1	2	3	4
	Stress															
Loading	15 8	1 82	1 98	2 00	—	—	—	155 8	157	157 7	—	30 500	298 700	298 100	97 000	291 500
	247 0	2 99	3 05	3 15	3 15	—	—	244 8	238	243	24 3					
	34 3	4 26	4 34	4 36	4 38	—	—	341 1	334	336	339 0					
	509 0	6 42	6 57	6 61	6 70	6 71	—	506 2	517	506	512					
	6 5 0	8 0	8 82	8 91	8·93	(8·95)	—	681·0	672	681	6 8					
							Mean Sq Resid	3 6	6 9	5 0	2 6					
Unloading	675	9 05	—	—	—	—	—									
	509	7 40	(7 31)	7 27	7 25	7 25	7 25	510 5	508 5	508 5	508 5	318 000	321 000	324 000	324 000	324 000
	342 3	5 31	5 25	5 23	5 21	5 21	—	343 5	343 5	344 5	345 5					
	247 0	4 12	4 08	4 00	4 00	—	—	248 5	250 5	247 5	248 5					
	151 8	2 90	2 83	2 80	2 78	—	—	150 5	150 5	1o2 5	151 3					
	0	0 95	0 73	0 71	0 69	—	—	—	—	—	—					
							Mean Sq Resid	1 9	1 2	1 32	1 8					

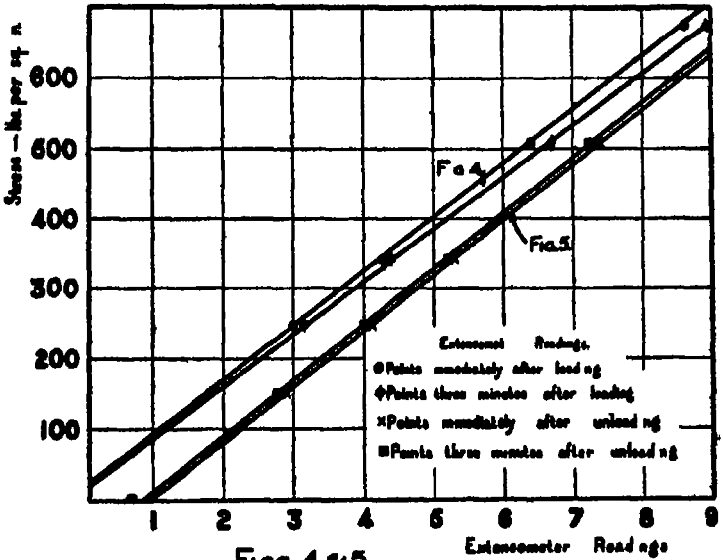
complete record of these measurements which will be analysed to show —

(1) The relation between *Stress* and *Extension* at the end of unequal periods of loading and unloading

(2) The relation between *Stress* and *Extension* for loading and unloading at (*a*) the instant these operations are completed, (*b*) after equal intervals of *Time*

(3) The relation between *Extension* and *Time* for the whole period of stress variation

(4) The relation between *Extension* and *Time* at constant stress for loading and unloading



Figs 4 & 5

(1) *Relation between Stress and Extension at the end of unequal periods of loading and unloading*

The most satisfactory way of demonstrating this relationship is by means of a curve In Fig 3 the values of the extensometer readings are plotted against the values of the weights on the lever of the testing machine Joining the points so obtained, it is seen that they do not lie on a straight line, and this want of proportionality prevents a true value of the modulus being calculated from the data obtained for unequal periods

(2) *Relation between Stress and Extension for loading and unloading at (a) the instant these operations are completed, (b) after equal intervals of time.*

Table IV. is a re-arrangement of the data of Table III. It shows the readings at given intervals of time—these intervals being measured from the time of applying the load. Further, it is assumed that the periods of time taken to load and obtain the first readings after each loading were equal, and for the sake of brevity have been termed " O " intervals Figs. 4 and 5 show the result of plotting the stress and readings for the " O " and 3 minute intervals. From these and similar lines, all fitted by the least square method, the calculated stresses and the values of the Modulus were found as given in the table In four minutes the value decreased from 305,500 to 296,500 lbs. per sq. inch for loading (from the figures it appears as if it would still get smaller with time) and increased in two minutes from 318,000 to 324,000 lbs. per sq. inch for unloading (from the figures it appears as if it had become constant).

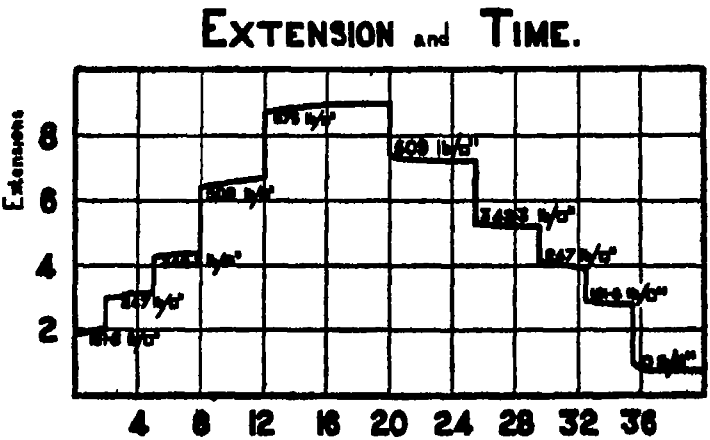


FIG. 6.

It should be noted that the initial values 305,500 for loading and 318 000 for unloading are both less than the values given by the previous two experiments, viz , 386,500 and 313,500 for loading and 416,000 and 336,500 for unloading

TABLE V

Stress		Time in Minutes.	Observed Extension (differences)	Calculated Extensions. X	Difference between Obs and Cal. Extensions	M S R
Total	Increase.					
675	166	0	(398)	385=A	[42·6=B]	3 6
		0 5	420	418	−2	
		1 0	422	427	+5	
		1 5	440	433	−7	
		2 0	440	439	−1	
		2 5	442	443	+1	
		3 0	444	447	+3	
		8 0	468	468	0	
509	166	0	(408)	397=A	[42 6=B]	4 0
		0 5	434	430	−4	
		1 0	438	438	+0	
		1 5	440	445	+5	
		2 0	446	451	+5	
		2 5	448	454	+6	
		3 0	464	459	−5	
		3 5	466	463	−3	
		4 0	466	466	0	
342 3 and 247 0	96 96	0	(222)	216=A	[20=B]	less than unity *
		0 5	232	232	0	
		1 0	238	236	−2	
		1 5	240	239	−1	
		2 0	242	242	0	
Both these stresses gave identical differences		2 5	244	244	0	
		3 0	246	246	0	
151 8	151 8	0	(302)	300=A	[30=B]	—
		1	334	330	−4	
		2	338	338	0	

* $X = A + Be^{[illegible]} - 0\ 1t$
Last term only influences one value, viz , 8 minute interval

(3) *The relation between Extension and Time for the whole period of stress variation*

The stepped diagram of Fig 6 is obtained by plotting time against extensions It shows clearly, how, with the application of a load the extension increases almost instantly at first and then rapidly falls off with time, tending to become asymtotic to a line parallel to the abscissæ (time). On the diagram the stresses are noted, which are well within the "so called" Elastic Limit. This term may now be defined as that stress which, if exceeded, the extension and stress cease to be proportional after equal time intervals of loading. Thus the stress at which such points as plotted in Figs. 4 and 5 cease to be linear will be the measure of the Elastic Limit.

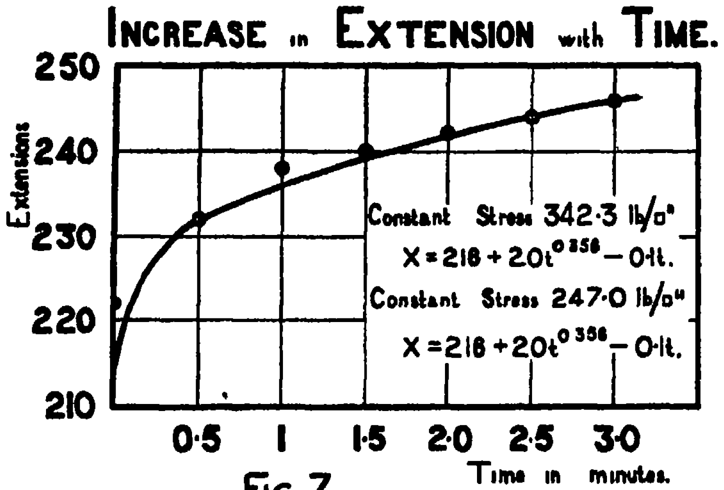


FIG. 7.

(4) The relation between Extension and Time at constant stress, for loading and unloading.

Loading.—An attempt has been made to find the laws of extension with time. Table V. indicates how far this has been successful, while Fig. 7 gives a typical time extension curve. The actual differences in the extensions for the standard length of 8 inches have been deduced from the extensometer readings for each constant stress and the results have been multiplied by 10^6. This gives a number of three figures which greatly simplifies the arithmetical calculations.

Several laws were tried of which the following proved the most successful.

$$X = B \, . \, t^n$$
$$X = A + B \, . \, t^n$$
$$X = A + B \, . \, t^n - f(t).$$

Although each of these can be made to fit the experimental values fairly well, it seems that only the last is permissible, the others giving infinite extensions with time, which is probably contrary to experience as after a certain period the material should come to rest at a definite extension, for these low stresses.

$$X = A + Bt^{0 \cdot 356} - 0 \cdot 1 \times t.$$

was the actual formula used for calculating the extensions at the constant stresses given in Table V. The last term of this law is very indefinite and only affects the calculated extensions in a small degree for cases of " t " greater than 6 minutes. It is to be observed that the same index 0·356 of " t " in the second term is common to the five sets of constant stresses. The constant " A " seems to depend upon the amount of the total stress, and " B " upon the magnitude of the increase of stress.

Unloading.—On comparing the recovery curves with those just considered it was seen that they were of a similar type, but further consideration showed that the same law could not apply. The contraction has commenced from some definite point and tended to proceed to another definite point, viz., that which it reached when the specimen was at the corresponding loading stress. Hence the required law should indicate this. After several attempts:—

$$R = X - A/t^n$$

was the form ultimately selected. In this if " t " is made large then " R " will become equal to " X " its starting point, on the other hand, if " t " be small " A/t^n " will be large and " R " will become small. The value " t " equal to " O " is not permissible as the conditions in the experiment are altering, that is to say, the load is being removed, which operation takes a certain amount of time to complete. The value " R " equal to " O " would correspond to the instant that all load had been removed and the corresponding value of "*t*" is the smallest allowable "X" is the extension from which recovery

TABLE VI

Stress		Time in Minutes.	Observed Extension Differences	Calculated Extensions R	Difference	M S R
Total	Decrease					
509	166	0	(330)	—	—	2 3
		0 5	346	346	0	
		1 5	348 ?	354	+6	
		2 0	356	356	0	
		2 5	356	356 4	+0 4	
		3 5	360	357 3	−2 7	
		5 5	360	358 1	−1 9	
				360 0=X	[A=7 94]	
342 3	166	0	(388)	—		2 2
		0 5	400	396	−4	
		1 0	400	401 5	+1 5	
		2 0	404	405 6	+1 6	
		3 0	408	407 0	−1 0	
		4 0	406	407 5	−1 5	
				410 0=X	[A=7 94]	
247 0	95 7	0	(218)	—	—	5 5
		0 5	226	230	+4	
		1 0	(226 ?)	236	+10	
		2 0	242	240	−2	
		3 0	242	241	−1	
				244=X	[A=7 94]	
151 8	95 7	0	(220)	—	—	1 7
		0 5	230	230	0	
		1 0	234	236	+2	
		2 0	240	240	0	
		3 0	244	241	−3	
				244=X	[A=7 94]	
0	151 8	0	(366)	—	—	2 0
		0 5	400	403	+3	
		1 0	410	408 5	−1 5	
		2 0	414	413	−1	
		3 0	416	414	−2	
				417=X	[A=7 94]	

commences and its values are equal to, or greater than, the experimental values This is parallel to the case of loading in which the values of "A" are slightly less than those obtained by experiment. Table VI compares the calculated and experimental values of ' R and Fig 8 gives a typical time recovery curve

TABLE VII —SUMMARY OF VALUES OF YOUNG s MODULUS OBTAINED FROM THE EXPERIMENTS

Exp No	Date	Modulus Value		Percentage Variation from Initial Values		Remarks.
		Loading	Unloading	Loading	Unloading	
1	7/4/22	386 500	416 000	—	—	Extensometer readings at 1 minute intervals after loading or un loading 1st time of loading
2	10/4/22	313 500	336 500	18 9%	19 0%	Second time of loading Modulus from mean line through points of nearly equal time intervals
3	11/4/22					Third time of loading Extensometer read ings taken at inter vals of — Minutes from loading or unloading—
		305 500	318 000	21 0	23 4	0
		298 200	321 000	22 8	22 9	1
		298 100	324 000		22 4	2
		297 000	324 000			3
		296 500	324 000	23 4		4

Maximum possible variation—28 8 per cent of initial unloading value
Variation from second loading to third loading (initial values of each)—2 55 per cent of second loading
Variation during the third experiment per cent of the first value loading—2 95 per cent that is in a period of 20 minutes
Variation during the third unloading per cent of initial value + 1 88 per cent

A summary of the alteration in the value of Young s Modulus as determined by the three experiments is presented in Table VII These variations are also expressed as percentages of its original value and are seen to lie between 19 and 29 per cent Such large variations are too great to be ignored However if the law of strain with time has been found the true value of the Modulus can be estimated for any of the conditions that are likely to arise in the optical methods of stress measurements, similar to those described in this paper.

IV.—OPTICAL PROPERTIES.

In the earlier work of 1914 a table was prepared showing the interference colours obtained by viewing between crossed nicols, celluloid while under direct stress. These colours, of which Table VIII. gives another list, are in the same sequence as those of Newton's rings, and similar to those given in the form of a beautiful chart by M.M. Michel-Levy and Lacroix in their work "Les Mineraudes Roches." On this chart each colour has against it a figure which represents a definite retardation. By matching the colours of the stressed celluloid with those on the chart it is possible to indicate in a general way the relationship between stress and retardation; vide Fig. 11. It should be observed that the colours repeat themselves, the number of repetitions being known as "orders." This approximate

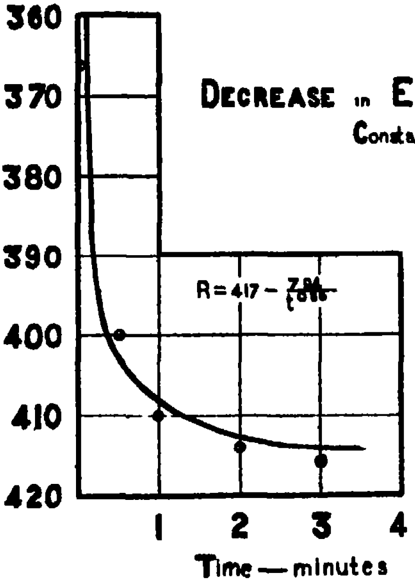


FIG. 8.

relationship not being of sufficient accuracy for the present purpose, more direct methods were employed, all of which depend upon retardation measurements. These measurements being somewhat novel they will be very briefly outlined.

A ray of polarised light in passing through a doubly refracting substance is divided into two, the ordinary and extraordinary rays, the transverse vibrations of which vibrate in two planes at right angles to one another; the vibrations having unequal phases. These orthogonal rays on being passed through a nicol prism become coplanar, but with the difference of phase maintained, with the result that interference takes place between them. The effect of this interference is colour. Retardation is the measure of this difference of phase or "lag," theoretically expressed by

TABLE VIII

Nominal Stress "f" lbs./sq. in.	Colour	Corresponding Retardation from Chart	Order "n"
0	Dark field (yellow)	40	1
188	Grey yellow	150	1
376	Light yellow	281	1
564	Yellow to orange	380	1
752	Orange	430	1
1,128	Brown red	500	1
1,316	Purple to violet	570	2
1,411	Green blue	650	2
1,505	Green	800	2
1,695	Green yellow	866	2
1,978	Yellow	910	2
2,355	Orange yellow	940	2
2,539	Purple	1,128	3

$$R = d(\mu_o - \mu_e) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where μ_o and μ_e are the indices of refraction of the two rays. This expression is a special case of a general physical law which will be stated when the "Single Mica Sheet Method" of stress measurement is considered. However, it cannot be employed for obtaining retardations on account of the experimental difficulties of measuring the indices of refraction in the celluloid specimen while under stress, but by the simple process of annulling the interference colour (retardation) in the specimen by an equal and opposite retardation, the value of which can be easily measured, this difficulty is overcome. The interference colour is compound, that is to say, is composed of all possible wave lengths of light with the exception of the one, the effect of which has been destroyed by the lag of one ray behind the other. This lag or retardation may be expressed generally by the formula

$$R = m\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where "m" is the order of the colour the wave length of which is equal to λ. λ is measured with the fixed arm spectrometer and "m" in the ordinary way would be equal to $\frac{1}{2}$ or a multiple of $\frac{1}{2}$. In the arrangement of the apparatus as used, the light was made to pass through two crossed nicol prisms besides the celluloid specimen. The last prism, known as the analyser, retards the light by an additional $\frac{1}{2}$ wave length, which thus causes interference to take place at a whole wave length. The value of "m" then becomes 1, or multiples of 1. If the nicols are rotated so that their vibratory axes are parallel, the effect of this additional $\frac{1}{2}$ wave length is counteracted.

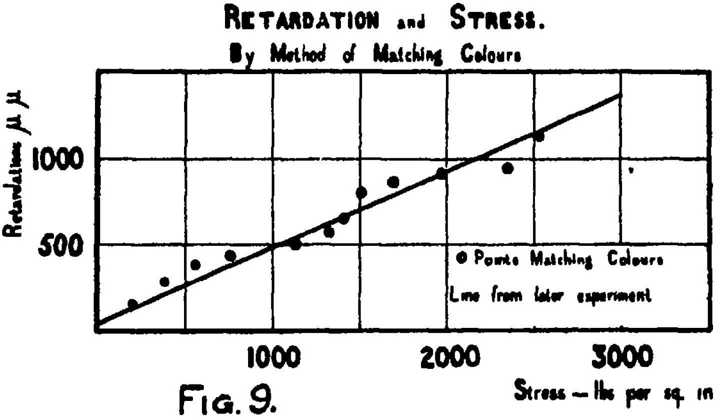


FIG. 9.

This method of measuring retardations has been found in practice to be reliable and accurate. In the remarks on apparatus it was stated that readings to plus or minus

TABLE IX —RETARDATIONS AND STRESS

Tension Experiment, 5th October, 1922

Specimen 0 17×0 262 inches

Time.	Load in lbs.	Nichols.	λ	m	Ret	Stress.		Constant C_t		Real Value from Curve	Remarks.
						Nominal lbs. sq in	Actual lbs. sq in	Nominal.	Actual.		
Hr Min											
6 11	63	×	676	1	6"6	1 420	1 380	2 83	2 93	2 96	Loading
6 13	124	×	676	2	1 376	2 790	2 750	2 87	2 93		,
6 24	56	×	587	1	587	1 260	1 220	2 76	2 84		,
6 26	110	×	587	2	1 174	2 480	2 440	2 80	2 86		,
6 33	44	×	498	1	498	990	950	2 99	3 11		,
6 35	90	×	498	2	996	2 060	2 020	2 86	2·94		
	20	|	588	½	294	462					,,
	84	||	588	1½	880	1 950					,,
	16		498	½	249	370					,
	70	||	498	1½	749	1 620					,,
6 14	64	×	676	1	676	1 440	Actual stress material withstanding obtained by subtracting initial compressive stress of 40 lbs per sq inch from curve R = 0 49f − 20 M S R of which is 23				Unload'g
6 28	54	×	587	1	587	1 218					
6 36	45	×	498	1	498	1 120					,,
	19	||	588	½	294	440					,,
	16	||	498	½	249	370					,,

2 $\mu\mu$ were possible, thus for the "first order" the retardations can be obtained to plus or minus 2 in 589 $\mu\mu$, taking the wave length of sodium light as the basis. Again for a higher order, say 4, the retardations would be within plus or minus 8 in 2,356 $\mu\mu$ (4 × 589). For yet higher orders this accuracy is hardly maintained on account of the dispersion of the interference bands which make it more difficult to judge the position of their centres.

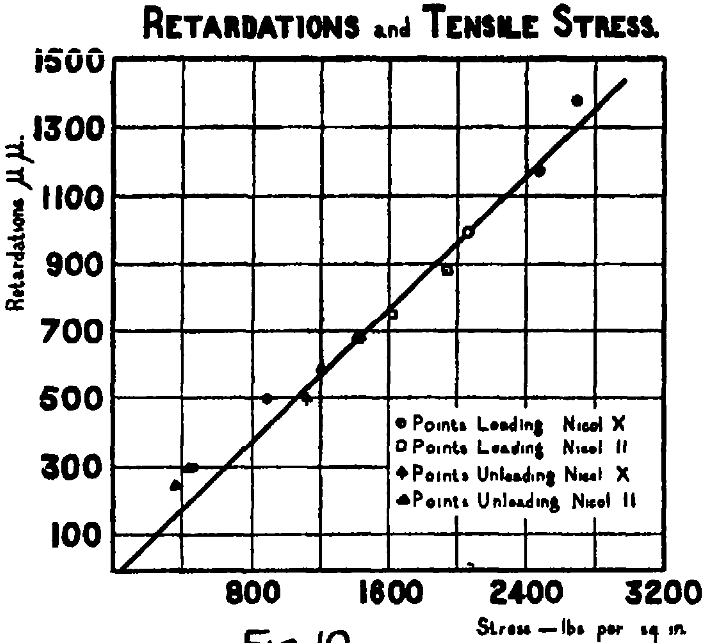


FIG. 10.

The difference between the two indices of refraction, or birefringence, can be obtained by equating the above equations for retardations. Its value for the Mica used in these experiments is 0·0056 and constitutes one of its permanent properties. The birefringence of celluloid on the other hand is not constant and is proportional to the difference of two stresses P and Q acting at right angles to each other, or more simply, as one of these stresses, (Q) is sometimes small, proportional to the direct stress (P).

Relation between Retardation and Stress.—The image of the specimen under a direct tensile or compressive stress was focussed on the slit of the spectrometer. The times, loads, wave lengths, and orders, for the tensile test, were all noted as recorded in Table IX The readings for loading with the nicols crossed, were alone used to obtain the curve of Fig. 10.

Table X and Fig. 11 give the corresponding measurements and curve for the compression test and on Fig. 12 the two curves are re-drawn on one diagram, reference to which will again be made. This diagram clearly shows that the rate of increase of retardation with stress is greater for compression than for tension, a result which has a very important bearing on optical stress measurements.

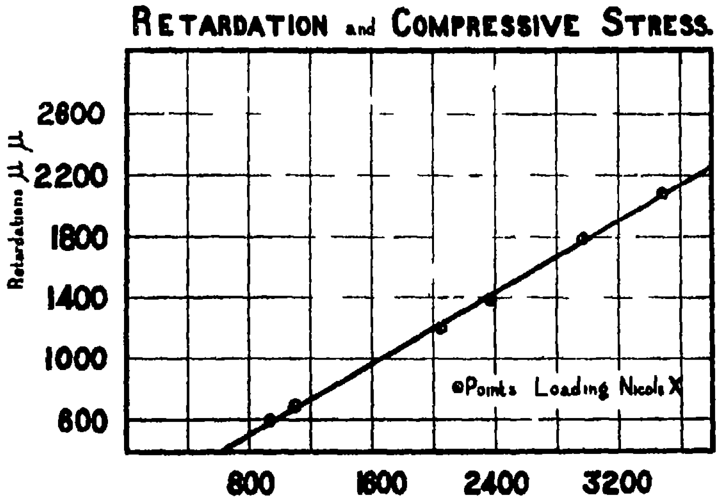


FIG. 11.

By taking the values of the retardations from the equations derived from these curves, viz.,

$R = 0{\cdot}49f - 20$ for tension (Fig. 10) . . . (3)

$R = 0{\cdot}58f + 35$ for compression (Fig. 11) . . (4)

and substituting them in the birefringence equation (1), the differences between the indices of refraction for either tension or compression "B_t" and "B_c" can be found for any stress within the range of these experiments.

TABLE X.—RETARDATION AND STRESS COMPRESSION EXPERIMENT, 17TH OCTOBER, 1922. Specimen, 0 277 × 0 168.

Nominal Stress. lbs /sq in	Actual Stress lbs /sq in	Retardations	Constant C_c			Remarks
			Nom	Act	Real	
935	995	596	3 70	3 55		Loading.
1,105	1,165	692	3 75	3 48		,,
2,040	2,100	1,192	3 50	3 36	3 49	,,
2,370	2,430	1,385	3 46	3 38		,,
2,970	3,030	1,789	3 60	3 52		,,
3,480	3,540	2,077	3 55	3 51		,,
2,210	2,270	1,385				Unloading.
1,700	1,760	1,192				
935	995	692				
595	655	596				

TABLE XI.—BEAM EXPERIMENT.

Stress lbs./sq ins	Tensile Retardation	C_t	Compressive Retardation	C_c
200	100	3 00	135	3·74
400	216	3 20	243	3 60
600	325	3 20	377	3·74
800	411	3 06	459	3·42
1,000	569	3 37	608	3 60
1,200	670	3 34	706	3·50
1,400	715	3 04	820	3·45
1,600	838	3 10	941	3·50
	Mean	3 16		3·57

Thus:—

$$B_t = (0\ 49f - 20)\ 1/d \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

$$B_c = (0{\cdot}58f + 35)\ 1/d \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

where "f" is the nominal stress in lbs./sq. inch, and d = thickness in inches. The fact that neither of these curves passes through the origin indicates initial retardation-stress. If this initial* stress be added algebraically to the nominal stress, the actual stress on the material is obtained. These latter plotted against retardations are as given in Fig. 12, and the modified birefringence equations become

$$B_t = (0{\cdot}49f)\ 1/d \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

and

$$B_c = (0{\cdot}58f)\ 1/d. \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

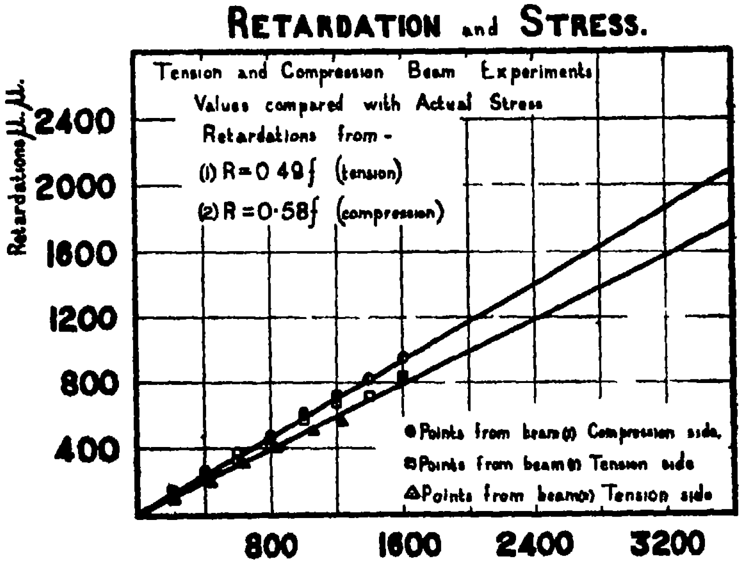


FIG. 12

As retardation is equal to the birefringence multiplied by the thickness, and the birefringence is directly proportional to the actual stress, it follows that the retardation is proportional to both stress and thickness; introducing a constant,

$$R = C.f.d. \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

where "C" is an optical constant, "f" is the stress and "d" the thickness.

---

* If uniform colour, at no load, indicates uniform initial stress, there must be an equal and opposite stress somewhere in the material. Optically this latter is very indefinite.

If 'f' represents the nominal stress then the two constants C_t and C_c vary slowly with the stress, but if f is the actual stress on the material then C_t and C_c are true constants with the values of 2·96 and 3·49 respectively see Tables IX and X

It was considered desirable to check these results and for this purpose the tension and compression sides of a beam under a uniform bending moment were examined. The retardations were again measured with the help of the spectrometer but in a slightly modified form the various positions in the beam being read off a scale fixed in the eye piece. Fig 13 indicates both the retardation and stress curves from which the points with the distinguishing marks in Fig 12 were obtained (beam 1). Both the tension and compression points lie close to the lines plotted from the direct measurements and another set of tensile values are also indicated taken from a later experiment (beam 2) in which the retardations were obtained by the "Mica Wedge Method." Further confirmation is furnished from the experiments of Prof E G Coker, D Sc, F R S, and

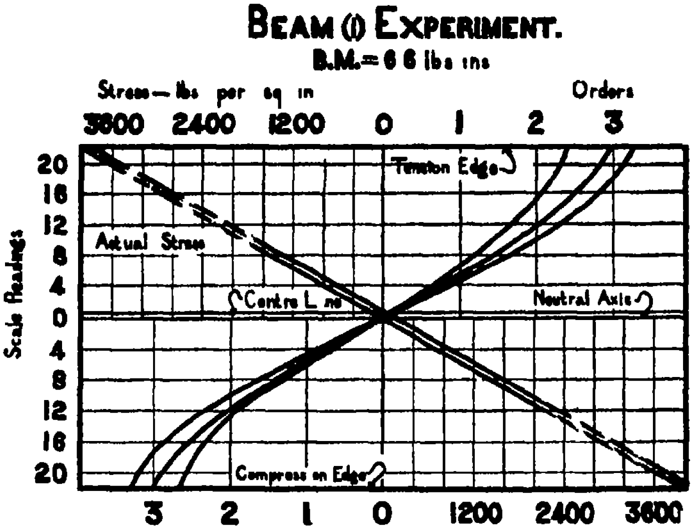


FIG 13.

K. C. Chakko, M.Sc, as described in their paper entitled "The Stress-Strain Properties of Nitro-Cellulose and the Law of its Optical Behaviour."[3] The uniform bending moment of 198 lb.-inches was selected as the necessary data is given for obtaining the retardations. Fig. 14 has been plotted from these values After the same restrictions were applied as those imposed in the author's experiments, viz., that no stress above 1,600 lbs. per sq. inch be considered, and that the stress up to this value be taken as being proportional to the distance from the neutral axis, then 2·61 and 2·96 are the values for C_t and C_c which gives a ratio of 0·885 as against 0·85 the ratio of the values obtained from the direct experiments of Tables IX. and X. and the ratio 0·885 (3·16 to 3·57) from the beam experiment of Table XI. It was not to be expected that the intrinsic

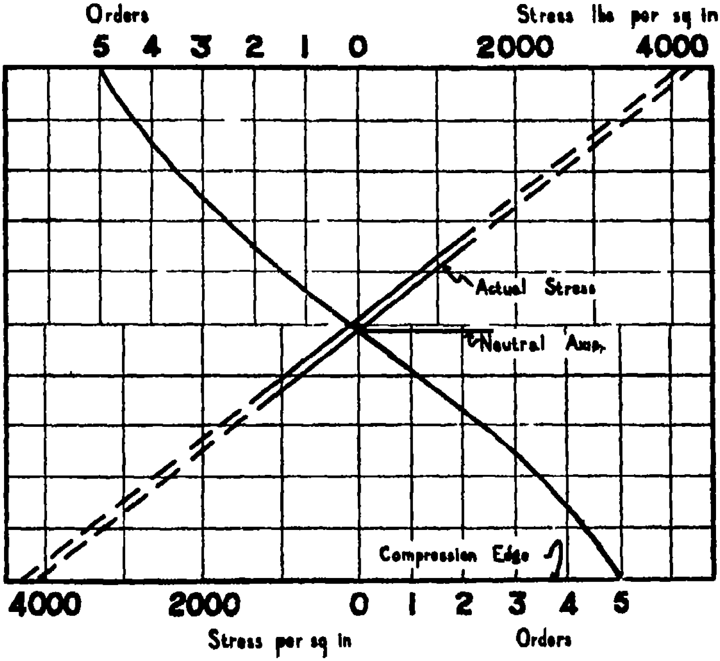


FIG. 14.

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[3] See *Phil. Trans.*, series A, vol. 221.

values of these constants C_t and C_c would be the same with the different materials, but the fact that their ratio is practically the same, is a strong confirmation that the optical properties differ for tension and compression.

Note.—The units in the equation $R = C.f.d.$ are as follows —
"R" is in $\mu\mu$ units.
"*f*" is in lbs per sq. inch.
"*d*" is in inches.

The rate of loading was not as regular as could be desired especially in the direct compression experiment as the apparatus had to be adjusted in order to obtain uniform stress as indicated by uniform colour.

V.—STRESS MEASUREMENTS.

There are several methods by which stress may be obtained. All depend upon the principle of annulling retardations.

(1) The method which has been greatly used[4] employs a standard tension specimen made from the same material as the specimen under examination. In this the retardations in the standard and specimen will annul each other if the direction of the tensile stress in the standard is arranged to be at right angles to the tensile stress, or parallel to the compressive stress, in the specimen, as the case may be. This method will be referred to as the "All tension" method. Before stress determinations are possible two important conditions must be fulfilled. They are:—

(*a*) That the properties of the material of the standard and that of the specimen must be identical, and that the thickness of both specimens must be the same

(*b*) That the rate of loading of the standard is the same as the rate of loading of the specimen.

With reference to (*a*). If tensile stresses are to be examined by a tension standard then this condition can be fulfilled, but if a tension standard be employed for the examination of compressive stresses, this condition can no longer be satisfied. The properties of the material in the standard and the specimen now differ and their thicknesses are unequal. The change in thicknesses is very small and can be neglected as a first approximation. The difference between the optical properties as shown by the values of the constants C_t and C_c are to great to be neglected. The method of averaging them is not recommended, although this reduces the error on the compression side, it introduces an equal error on the, otherwise correct, tension side.

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[4] See *Phil. Trans.*, series A, vol. 221, and other papers by Prof. E. G. Coker, F.R.S.

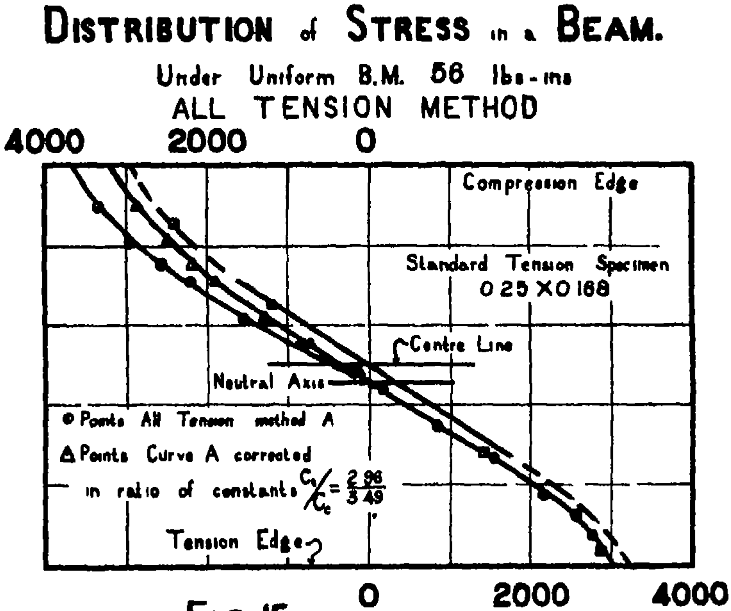


FIG. 15.

The following correction, if applied to the compression side as examined by the "all tension" method will give as accurate results as for the tension side.

Fig. 15 is the result of an experiment made on a beam of 0·75 by 0·17 inches supporting a uniform bending moment of 56 lb.-inches. It has been shown that a greater tensile than compressive stress is required for the same retardation, and further that these are then in the ratio of the constants C_t to C_c, *i.e.*, 2·96 to 3·49. If then compressed retardations be annulled with a standard tension member (the retardation in both standard and specimen being of the same value), the stress as measured by the load on the standard, must be reduced in this ratio. Curve "A" in the figure shows the compressive stresses as obtained by the "All tension" method, and Curve "B" these values corrected in the ratio of 2·96 to 3·49. It is seen that the curve, so altered, is now continuous and symmetrical. To check this distribution of stress this continuous curve was integrated, with the result that the internal moment of 55·5 lb.-inches was obtained, as against the applied bending moment of 56 lb.-inches.

A very important and fundamental principle is involved in this experiment. In the previous beam tests the stresses which correspond to the measured retardations were confined to values within the elastic limit, but in the direct tension and compression tests (Figs. 10 and 11) the stresses were much higher, in fact were taken to values approximating to double this value. It is seen—and this is the important part—that these higher stresses are still linear with the retardations. How far this law actually holds the Author has not as yet attempted to discover. In the beam test of Fig. 15 the parts of the curve for stresses in the neighbourhood of 1,600 lbs. per sq. inch, begin to bend more and more inwards as the outer edges of the beam are approached. The retardations at these places in the beam have been annulled with a standard under direct stress, which stress has been proved to be linear with its own retardations, hence the retardation at any point in the beam must also be directly proportional to the stress at the same point. From the bends in the symmetrical curve of Fig. 15, it therefore follows that the stresses beyond 1,600 lbs. per sq. inch are not proportional to their distances from the neutral axis. Thus a wide field is opened up for determining the distribution of stresses which exceed the elastic limit.

(*b*) The second condition can only be partially satisfied in tests other than simple direct stress determinations, which are very seldom required in practice. When a beam is loaded the extensions of its various layers are subject to time; the longer the load is applied the greater these extensions become, unless the point of equilibrium has been reached. If the retardations in the beam are annulled with a celluloid standard, the time intervals of loading of the latter can only be the same as that of the beam for the first application of the loads. With an additional load applied to the standard it is by no means certain that the proportion between the resulting retardation and the new stress in the standard will be the same as the proportion between the equal and opposite retardation in the beam and the corresponding stress in the beam. By delaying the readings until three or four minutes after loading the standard, the effect of unequal time intervals is reduced. This will be better understood if reference is made to the curves connecting extension with time at constant stress (Section I.), where it will be seen that the greater portion of the extension has already taken place for similar periods.

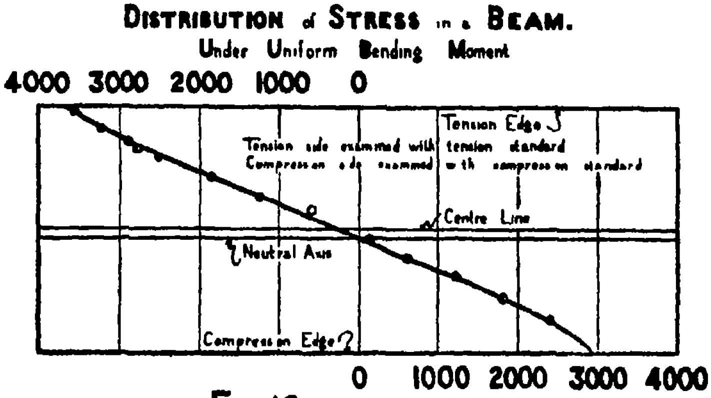


FIG. 16.

(2) The next method of measurement is the same as that adopted in the earlier experiments. This consists in annulling tensile retardations with those of a tension standard and annulling compressive retardations with those of a compression standard. Fig. 16, the result of a beam test carried out on these lines, shows that the tensile and compression portions of the stress distribution curve are continuous and symmetrical. Although this method satisfies

condition (*a*) it is very difficult to control, and has the disadvantage of having to alter the testing machine from tension to compression to accommodate the two standards. To satisfy condition (*b*) similar precautions to those adopted in the "All tension" method have to be taken.

(3) Direct measurement of retardations with the spectrometer are only possible in simple cases of stress determinations and for the more complicated cases the two following methods give good results.

"*The first*" depends on annulling the retardations in the specimen by means of the retardations of Mica sheets of definite thicknesses, and will be referred to as the "Mica Wedge" method. The relative retardation of each sheet was very accurately measured, care being exercised to correctly orient each sheet at 45 degrees with its extinction direction. These sheets were substituted for the standards. The advantages of the "All tension" method were thus retained, with the additional gain of having a real standard the properties of which are constant and entirely independent of time; by the simple process of placing these sheets over each other, the retardations may be added, or subtracted, according to whether the sheets are arranged parallel to, or at right angles to, their edges, which were all cut at 45 degrees to their extinction directions. On several of the figures will be found points which have been obtained by this process and are given as affording another check on the work By judicious selection of the various thicknesses the interval in the value of the retardations which these plates will give can be made as small as necessary and it was found that intervals of 100 $\mu\mu$ were sufficient for most purposes. If the object of an investigation is stress distribution it is not imperative to know the proportion between retardation and stress, if, on the other hand, the actual stresses are required, then experiments similar to those already described for direct tension and compression, or beam tests, must be made with the same material—if possible with the same specimen. Often, however, the retardation or stress is wanted at some particular place and the mica sheets available have not the requisite thickness.

In "*the second*" method this difficulty is removed by adopting what may be termed the "Single Mica Sheet"

TABLE XII.—RETARDATIONS AND ANGLE OF INCIDENCE FOR MICA SHEET (11+11) ROTATED ABOUT β AXIS, 8TH JANUARY, 1923

Indices of refraction of mica used—α =1 5609, β=1 5941, γ =1 5997

Mean of Obs Angles.	Retardations	
	Obs	Cal
0 — 0	1,863	1,863
20°—30′	1,242	1,251
29°—45	621	604
36°—54′	0	0 Optic Axis Angle in air
42°—37′	621	—
44°—26		732
48°—58′	1,242	—
53°— 8	—	1,880
54°—36′	1,863	—
60°— 1	2,484	—
64°- 10		3,110
66° 46	3,105	
80 — 0	—	4,150
90°— 0	—	4,400

TABLE XIII.—RETARDATIONS AND ANGLE OF INCIDENCE FOR MICA SHEET (11+11) ROTATED ABOUT γ AXIS, 28TH JANUARY 1923

Observed Angle (one reading)	Retardations	
	Obs	Cal
0°	1,863	1 863
10	1,990	2,012
24°	2 624	2,670
40°	3,760	4,010
60°	5,800	6 232
80°		7,960
90°	—	9,360

method. By rotating a mica sheet about its (γ) vertical axis or its (β) horizontal axis the uniform colours (retardations) alter, increasing in value in the former case and decreasing at first in the latter case, and then after a certain angle known as the optic axis angle in air has been reached increasing in value until it becomes larger than its normal

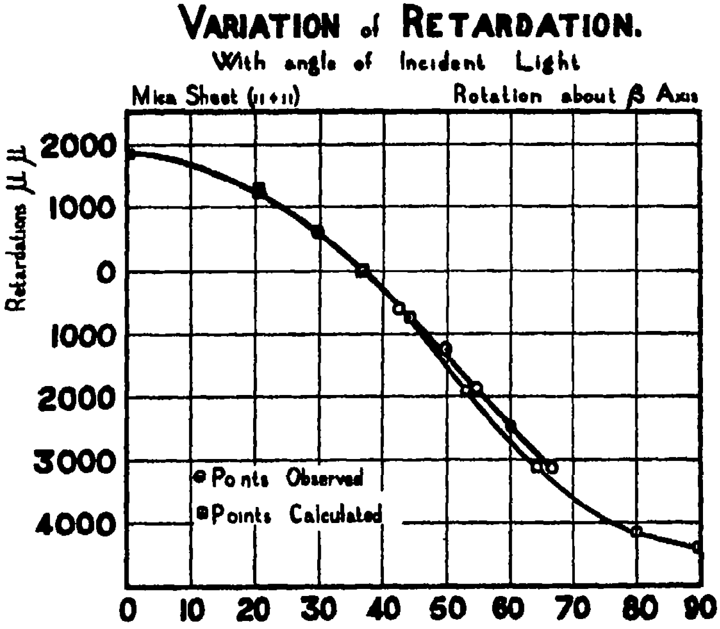


FIG. 17

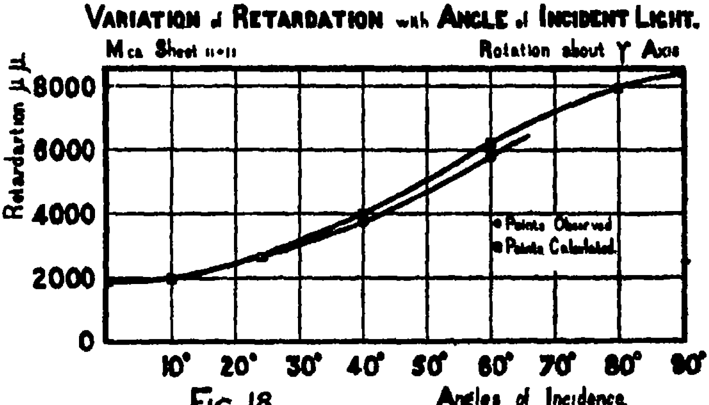


FIG 18

value Tables XII and XIII and Figs 17 and 18 give the values and curves obtained both from experiment and calculation, the latter from the theoretical laws given below, for which the Author is indebted to Mr. R. G. Lunnon, M.A., B.Sc. It is these laws of which the birefringence equation (1) is a special case. (See Appendix I.)

By mounting the mica sheet on a universal stand in which angles can be measured to one minute or less, continuous values of the retardations to practically any desired accuracy can be obtained. By this simple means retardation measurement is reduced to the extremely convenient operation of measuring angles.

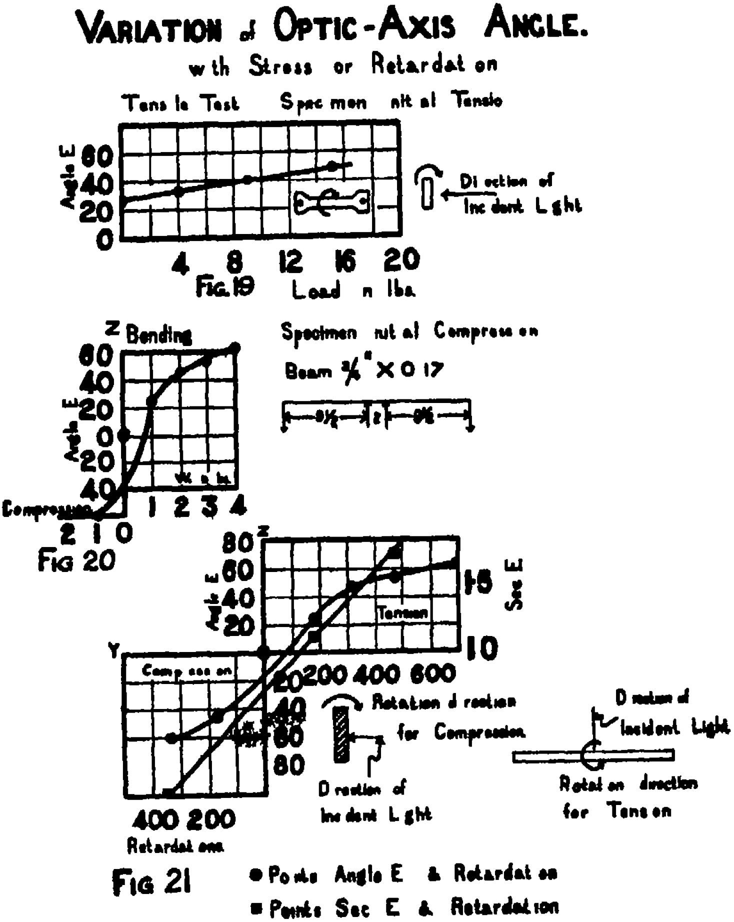
VI.—FURTHER OPTICAL PROPERTIES.

The similarity of the phenomena when stressed celluloid or mica are examined under polarised light led the Author to conclude that there must be some closer connection between these substances than it was customary to think. Mica is a bi-axial crystal, which means that it possesses three principal indices of refraction α, β and γ, α being the least and γ the greatest, β being of an intermediate value, the nearness of which to the others determining the sign of a crystal. For instance if the value of β is nearer to that of γ, as in the case of mica, then the crystal is negative, if on the other hand β is nearer to α the crystal is positive

With strained celluloid the problem resolved itself into two parts (1) To discover if it behaved optically as a bi-axial crystal, and if so (2) what measurements could be made to substantiate this. The most obvious, viz., to measure the three principal indices of refraction is extremely difficult, but nevertheless, is being attempted for a reason which will become apparent later.

The next attempt made was to examine a piece of strained celluloid under convergent polarised light. This actually showed that the particular specimen used behaved as a bi-axial crystal. The characteristic figures, however, were very indistinct, and all that could be said was that the "brushes" moved in the correct manner.

This was quite sufficient to indicate the presence of the two optic axes, and it remained to devise means by which the angle between these could be estimated The definition of an optic axis viz —that direction in which light passing through a crystal suffers no double refraction—suggested the following method which is very similar to that which



had already been adopted for measuring the retardation variation with the angle of rotation in the single mica sheet experiment

A beam of parallel polarised light was arranged to fall on the specimen of strained celluloid in increasing angles of incidence by rotating the specimen and in the positions of maximum darkness the angles were noted These positions of maximum darkness correspond to the direction of no refraction i e to the optic axis angle It was found that these angles altered considerably with stress, the alteration being very rapid at first

TABLE XIV —OPTIC AXIS ANGLE AND LOAD

Tensile Test Specimen 0 264×0 17 inches

Material with initial tension

Obs Angle	Load in lbs	Remarks
27°	0	Initial state of material
33°	4	The load was applied in the same direction as the initial stress
40°	9	
48°	15	

The first quantitative experiment made was with a standard tension specimen the material of which possessed an initial tensile stress Fig 19 and Table XIV show how the angle changed as the tension increased In the experiments only half the angles between the optic axis

TABLE XV.—OPTIC AXIS ANGLE AND LOAD OR RETARDATION

Bending Test —Specimen 0 75×0 17

Material with initial compression

Obs. Angle	Wt. in lbs on beam	Retardation	Remarks.
45°	0	170	Compression
60°	1	335	
25°	1	175	Tension
47°	2	310	
54°	3	470	
64°	4	692	

were measured as the whole angle is symmetrical about the normal position and are recorded thus in the Tables.

The second experiment was made with a beam having initial compression. Both the compression and tension sides were examined at particular distances on either side of the neutral axis and all measurements given in Table XV refer to these two positions.

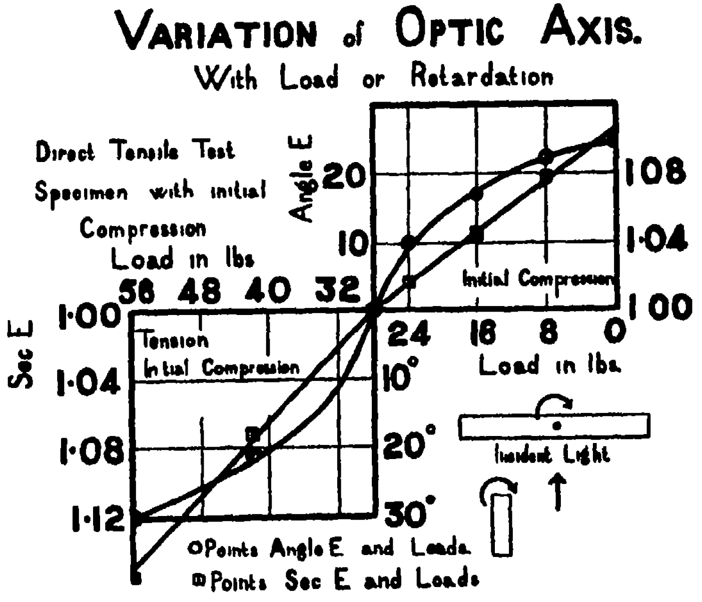


FIG. 22.

The increase of stress was obtained by increasing the bending moment on the beam and the retardations were measured by the mica wedge method. It was found that the optic axis angle for the tension side was in a plane at right angles to that containing the optic axis angle for the compression side. This fact not only strengthens the argument that strained celluloid behaves as a bi-axial crystal but places the phenomena amongst those belonging to crystals, the axial planes of which are inverted under suitable conditions. Figs. 20 and 21 show the curves obtained by plotting the measured angle against either the load on the beam or the retardations On the figures small diagrams are added to show the relative positions of the incident light and the specimen

VARIATION of OPTIC-AXIS ANGLE.
with Stress or Retardation
Beam Test (Tension side only)

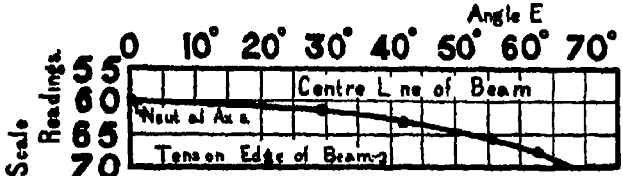


FIG 23

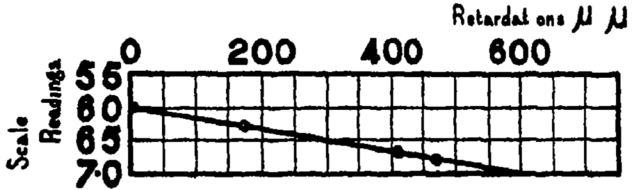


FIG 24

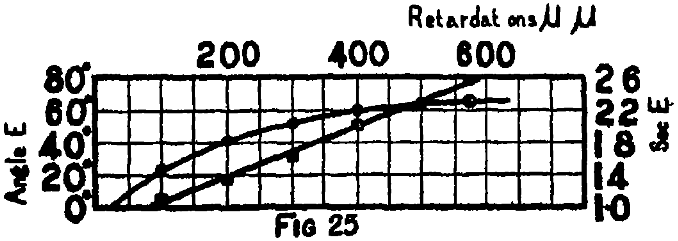


FIG 25

•Points Angle E & Retardations

■Points Sec E² & Retardations

As a check on this important result the same specimen was used in a direct tension experiment The loads were put on at first so that the initial compression was slowly reduced until quite counteracted Fig 22 the resulting curve is obviously of the same character as that already shown Further, the secants of the angles are plotted against the load in lbs with the result that two straight, or approximately straight lines are obtained and the interesting fact emerges, that the inclination on the tension side is greater than that on the compression side, a similar conclusion to that already obtained from the direct tension and compression tests of Figs 10 and 11

The third and final test was a re-trial of the beam experiment using again the same specimen with initial compression In this the actual retardation measurements were made for the tension side only of which Fig 24 is the graph Another curve is given in Fig 23 in which the angle is plotted against the various positions in the beam By connecting the information from these two independent experiments, the required relation between retardations and the angle is obtained and is given in Fig 25 where it is seen that the curve is becoming less steep as the value of the angle increases

Table XVI —Optic Axis Angle and Load

Tension Test —Specimen, 1″×0 17″
Material with initial compression

Obs. Angle	Load in lbs.	Remarks.
24°—30	0	Initial compression
22°— 0	8	,, ,,
17°— 0′	16	,, ,,
10°— 0	24	,, ,,
0°— 0′	28	,, absorbed
1°—30	30	Tension
21°— 0	42	,,
30°— 0′	56	,,

Preliminary analysis of the last three experiments.—Had it been possible to have measured the three principal indices of refraction for the different stresses the various values of the optic axis angle in air could have been checked from the relation

$$\operatorname{Sin} E = \alpha \sqrt{\frac{\gamma^2 - \beta^2}{\gamma^2 - \alpha^2}}$$

where " E " is equal to one half of the optic axis angle in air and α, β and γ are the three indices of refraction which alter with stress

TABLE XVII —OPTIC AXIS ANGLE AND RETARDATION

Beam Tests Specimen 1″ × 0 17″
Material with initial compression

For Fig 34.		For Fig 35	
Obs Angle	Scale Readings giving position of annulled stress in beam	Retardation	Scale Readings giving position in beam
0	6 00	0	6 00
24°–30	6 15	175	6 30
42°– 0	6 35	410	6 70
55°–30	6 60	470	6 80
68°– 0	6 80		

VII —CONCLUSIONS

(1) In the transparent substance celluloid strain is not only a function of stress but is a function of the time the stress has been applied

(a) $X = A + Bt^n - f(t)$

gives the extension after the load has been applied " t " minutes, the time being reckoned from the instant that the full load has come upon the specimen A is a constant which may be looked upon as the instantaneous extension under a given stress If the corresponding value of Young's Modulus be taken " A " may be calculated in the ordinary way " B " is another constant and seems to depend upon the magnitude of the difference of stress while " n " seems to be a true constant, whose value as given by these experiments is equal to 0 356 $f(t)$ is some function of the time whose value is as yet indefinite.

(*b*) $R = X - C/t^n$

gives the contraction (recovery) after the load has been removed “ t ” minutes. “ X ” is the extension from which recovery commences and “ C ” is a constant which appears to be independent of stress. “n’ is another constant whose value is equal to 0·86.

It should be noted that “ X ” and “ R ” give the increase and decrease in extension with time for a series of consecutive loadings or for a single loading.

(2) Extensions and contractions are proportional to stress only after equal periods of loading or unloading.

(3) The value of Young’s Modulus depends upon the previous treatment of the material. Generally it decreases with use, but in any given experiment the unloading value is greater than the loading value.

The maximum variation obtained in the experiments was −28 8 per cent., and applies to the period covering the three experiments. In the third experiment alone, the maximum variation was 8·4 per cent.

(4) Stress and retardations are linear for equal intervals of time.

(5) The proportionality of stress to retardation differs for tensile and compressive stresses. The ratio of the optical constants C_t to C_c has an average value of 0·865.

(6) The “ All tension ” method for compressive stress determination must be corrected in the ratio of the constants C_t to C_c.

The effect of time can be reduced by delaying taking observations until a few minutes after loading.

(7) The substitution of Mica for the material of the “ standard ” leads to accurate measurements of retardations. If the law of stress-retardation with time be known from direct experiments, determinations of stress for complicated cases are possible.

(8) The “ Single Mica Sheet ” method is an extremely sensitive and accurate way of measuring retardations. It possesses the enormous advantage over other methods in that the values of the retardations may be checked by the physical laws given in Appendix I.

It also possesses the asset of simplicity as retardation measurement is reduced to the mere process of measuring angles.

(9) Strained celluloid behaves as a bi-axial crystal. This follows from:—

(*a*) The movement of the "brushes" when viewed under convergent polarised light.

(*b*) The presence of the two optic axes and their measurement at constant and varying stress.

(*c*) The inversion of the optic axis plane in passing from tension to compression.

Another confirmation may follow from the measurements of the principal indices of refraction. The optic axis angles calculated from these should check the values found by experiment.

The importance of finally establishing the behaviour of strained celluloid when examined under polarised light cannot be over-estimated. Routine work for Engineering purposes cannot proceed with any certainty until the probable errors of the various methods are better known and appreciated.

The Author wishes to state his great indebtedness to Prof. Engineer-Commander C. J Hawkes, R.N. (retired), M Sc., for granting the necessary facilities and to many other of his colleagues of Armstrong College in the University of Durham, especially to Mr R G Lunnon, M.A., B Sc., for their kind help and criticism as the work proceeded, and finally to his Senior Students particularly to Mr E. W. Houston, B Sc., for his enthusiastic help in taking the necessary readings.

APPENDIX I

THEORETICAL RELATION BETWEEN RETARDATION AND THE ANGLE OF INCIDENT OF POLARISED LIGHT FOR MICA

d – thickness of mica sheet in m m

μ_e & μ_o indices of refraction for extraordinary ray and ordinary ray respectively

θ & θ – angles which ray paths in mica sheet make with normal

R – Retardation

then $R = d[\mu_o \sec\theta \quad \mu_e \sec\theta^1 - \sin\iota\,(\tan\theta \quad \tan\theta^1)]$

When the mica sheet is rotated about its β axis and from Fletcher's indicatrix (ellipsoid of indices of refraction)

$$\mu_i = \frac{\alpha \quad \gamma}{\sqrt{(\gamma \quad \alpha)\sin \quad \theta^1 + \alpha^2}} \text{ for which } \sin\theta = \frac{\alpha \quad \sin\iota}{\sqrt{\alpha \quad \gamma^2 \quad \sin \quad \iota(\gamma^2 - \alpha^2)}}$$

Again $\sin\theta = \frac{\sin\iota}{\beta}$ where $\beta \quad \mu_o$

$\sin\theta = \frac{\sin\iota}{\mu_e}$ where μ_e – index of refraction which is varying with the angle of incidence

When the mica sheet rotates about its γ axis

$$R = \alpha\,[\mu_e \sec\theta' - \gamma \sec\theta - \sin\iota\,(\tan\theta - \tan\theta)]$$

where $\sin\theta = \frac{\alpha \sin\iota}{\sqrt{\alpha\,\beta - \sin^2\iota\,(\beta^2 \quad \alpha^2)}}$

$\sin\theta \quad \frac{\sin\iota}{\gamma}$ where $\gamma = \mu_o$

$\sin\theta = \frac{\sin\iota}{\mu_e}$ where μ_e = index of refraction which is varying with the angle of incidence

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THE BIOLOGY OF *THRINAX MIXTA* KL AND *T MACULA* KL

By A. D PEACOCK M Sc.

[Received July 1923.]

In the Entomologists Monthly Magazine of July 1920 the late Dr Chapman gave notes on the life history of *Thrinax mixta* the larvæ of which had been obtained at Albury Surrey Since then I have found it and its near ally *T macula* in the North of England and have reared both species but while certain of my observations corroborate or supplement Dr Chapman s many new features have been noted

Larvæ of both species were found in good numbers generally—as many as 8 10 at one beat from a clump—on *Athyrium felix femina* as follows —

June 9 1921 : in the grounds of Ravensworth Castle Co Durham

June 14 1921 a few specimens in a mixed wood at Winlaton Mill Co Durham

June 19 20 1921 in a pine wood on Roughside Moor Co Durham

June 21 1921 in a mixed wood at Prudhoe Hall just on the Northumberland side of the Northumberland Durham border

Enslin states that the development of *Thrinax* species is unknown so that Dr Chapman s and many of my notes on *mixta* are new as likewise are mine on *macula* the larva of which according to the same authority is unknown In view of the significance of these facts to the collector and systematist I deal first with the differentiation of the two types of larva but as my rearing experiments were principally conducted for obtaining cytological material I am only able to treat of the matter briefly and generally

Differentiation of the Larvæ.

	mixta	*macula*
Colour	Head dorsally black, smooth, polished but posteriorly with a narrow lighter region so that the posterior edge of the black presents two large semicircles, one on either side of the middle line, front aspect black except for clypeus and cheeks, which are very light brown, and the labrum which is light brown, eyes black	Head light brown, eyes black
	Body dorsally green, laterally and ventrally almost white, rump has two large black spots	Body dorsally a soft, somewhat light green, the gut when full showing dark green along the middle half of the back, laterally (through lens) very light yellow, almost white, ventrally almost white, no rump spots
Size	Full grown, 20 mms long, 2 5 mms broad (*Cf* Chapman, who gives 21 mms for a large specimen)	Full grown, 15 mms long 2 mms broad
Pupation colour change	About three days before burrowing the middle third of the body shows brown owing to colour of fat body	Dorsally—anteriorly and posteriorly for about one fifth of length, a slaty green, median portion brown owing to colour of fat body, laterally and ventrally pale, slaty blue while, in addition, due to colour of fat body, the belly may appear suffused with pink which gives it a pale lilac colour

The characteristic points of differentiation are the presence or absence of the double semi-circular border on the occiput, the size, the colours during feeding and just prior to pupation, and the presence or absence of the two black rump spots

LIFE HISTORY

	mixta	*macula*
Dates of imaginal emergence	From April 29 May 8, 1922	May 8 17, 1922
No of eggs per female	20 30	?
Incubation period	10 days	8 days (1 experiment)
Larval period	15 21 days	17 days „
Pupal period	10 11 months	10 10½ months
Longevity of imago	♀7 11 days ♂8	♀11 days

From this summary and other facts to be noted, *mixta* and *macula* are obviously " parallel species " such as exist among the Lepidoptera, *e g* , the autumnal moths *Oporabia dilutata* and *O autumnata* and the daggers *Acronycta tridens* and *A psi*

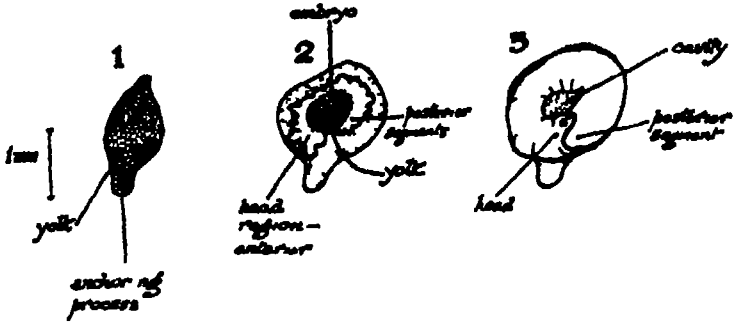
Technique In these breeding experiments the collected larvæ, which were mostly well advanced were easily reared on cut fern placed in water kept in glass jars having the open end covered with fine muslin When the larvæ were judged full-grown a mixed litter of damp peat, coconut fibre and sand and pieces of soft wood were placed in the rearing jar, both materials being used in case the larvæ had a particular preference A few specimens of both species did make use of the litter and metamorphosed, but the greater majority used the wood The wood and its contained larvæ were kept in closed glass jars or tins over the winter At the time of imaginal emergence the local ferns were still uncurled but potted plants each covered with a large glass vessel proved quite suitable for experiment Both species proved readily parthenogenetic

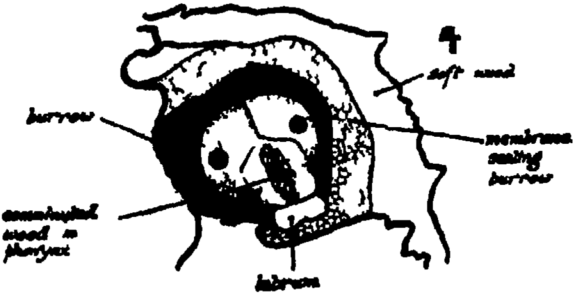
Egg-laying and Incubation So far as observations went there seems no great differences between the species The female takes its position on the under side of the leaf of the uncurling fern—this surface, of course at this period being the exterior portion of the leaf—and bends the posterior half of abdomen almost at right angles to the rest of the body The saw is protruded in a direction almost in the same line as the long axis of the flexed posterior of the abdomen and the act of sawing is deliberate, somewhat slow and is assisted by lateral abdominal movements. This method is different from that used by such slit and groove-making saw-flies as I have observed in which the saw is like a radius the tip of which moves in the arc of a circle. The reason for this difference is apparent when one observes that the egg is placed in position in a peculiar manner. The procedure is that a very short slit-like perforation is made in the main vein, or one of its branches, from the under surface so that the fluid egg is almost entirely expressed through the hole and comes to lie in the upper (inner) face of the leaf. But, when most of the egg has been passed, the withdrawal of the ovipositor is made in such a way that a small portion of the egg is retained and nipped in the slit. This portion resembles in profile the ballast-keel of a racing yacht. Dr. Chapman had evidently not witnessed this operation for, while he recorded this curious relationship between egg and leaf, his suggestion of how it comes about was erroneous. The rationale of the mechanics is explained therefore by the facts that the " saw " is used more as a " borer " rather than a " slitter."

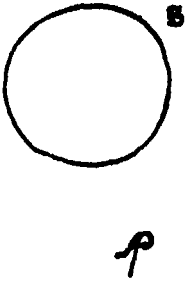
Two curious features present themselves therefore, firstly the " placenta-like " relation between egg and leaf to secure osmosis for the incubational swelling and, secondly, the protection of the egg, in the early period at least, by the folds of the leaflet instead of by the internal tissues which surround the egg laid in a cavity of leaves as in so many other species.

The dimensions of the newly-laid egg of *macula* are: long axis 1·6 mm., broad axis 0·7 mm., side to side axis 0·7 mm. The egg towards the end of incubation may attain for these axes respectively 1·75 mm., 1·2 mm. and 1·0 mm. (See Figs. 1-3.)

The incubation of *macula* is illustrated on page 369 and shows the shape and the positions assumed by the embryo at various stages in development and also the great increase in volume of the egg during the process. The stages could be distinguished quite clearly with the aid of a binocular dissecting microscope during the fixation of the eggs in Carnoy's fluid but, as the fixative appeared to shrink the specimens—the embryos at least—their actual dimensions should be a little larger than those figured the embryos too become detached from the shell usually Unfortunately I was unable to obtain full data relating to the ages of the







Figs 1—3 Eggs of *Thrinax macula* showing in profile stages in development anterior to left posterior to right Fig 1 newly laid egg showing tapered upper pole and anchoring process completely filled with yolk Fig 2 egg containing developing embryo nearly surrounding the residual yolk; fixation has shrunk the embryo away from the shell Fig 3 larva in egg nearly ready for hatching the posterior abdominal segment almost hides the head Note in 2 and 3 how the egg has increased in volume.

Fig 4 Larva sealing mouth of burrow

Fig 5 How the sealing membrane is laid down The dotted lines represent roughly the position of the membrane margin at successive stages the inner eccentrically placed circle is the last area to be sealed.

eggs concerned It should be noted that on laying the yolk also fills the egg peg but disappears from it as development proceeds The orientation of the egg and embryo may best be discussed in relation to the shape of the egg and its peg; the anterior of the egg is the region in which lies the anterior of the embryo, the peg at this aspect having almost a straight edge; the posterior of the egg bulges outwards and contains the posterior portion of the embryo while the posterior edge of the peg is convex. In profile the set of the egg on the peg resembles that of the human head on the neck; the rounded point of the peg is directed downwards and somewhat anteriorly. In one instance the embryo occupied a reverse position in the shell, and in my only example of *mixta* the embryo lies upside down with the head end directed anteriorly. How far this is typical of *mixta* I cannot say.

Larval Life. The newly hatched larva of *mixta* has a clear translucent head, black eyes and brown-tipped mouth parts; the body is opaque white and dorsally, along the middle third of the gut, there is a streak of orange marking the position of what is probably residual yolk from the egg. In my experiments, despite the facts that the living plants were covered by glass vessels and undisturbed except for daily inspection, a large number of young larvæ died through falling from the leaves to the soil and there drying. This feature I have observed to occur in other species in the early part of the year, *e.g.*, *Pristiphora pallipes* and conclude that, if this is what happens under sheltered conditions, the mortality in nature from this cause must be considerable.

On one occasion when unfamiliar with their habits, I witnessed a striking and curious phenomenon in one of the large breeding jars where about 50 larvæ were reared on fronds of fern. Chancing to look at the jar one afternoon I saw the insects crawling rapidly in all directions on the sides of the jar and over the food plant in a high state of excitement—feverish it seemed. As they were well-grown and declined to feed I surmised they were in a migratory and pupation frenzy, so placed pieces of soft wood in the jar. The larvæ immediately settled quietly on the wood and commenced boring.

The interesting operation of boring was observed in the case of *macula*. Two larvæ commenced tunnelling a piece of soft beech in the morning and after about 7 hours had excavated to such a depth that only their rumps were showing. A few minutes later one was in a reversed position working with its mouthparts at the entrance to the burrow; the other did not reverse until 2 hours later. The precise method of reversing I did not observe but it was so speedy that I am inclined to think that the larvæ turned in the burrows and did not forsake their shelter at all. Once only one of them retreated until its posterior half protruded but possibly the movement was only for the removal of sawdust. After reversal one larva put finishing touches in shaping the mouth of the burrow and then proceeded to seal it. These operations were observed with a binocular dissecting microscope. The caterpillar nibbled and undercut the wood just inside the mouth of the tunnel, the sawdust being stored in the pharynx. Silk was next extruded from the labial pap and, simultaneously, comminuted wood extruded from the mouth and, by the working of the mandibles and maxillæ against the flattened labrum, all the mouthparts fashioned a portion of a thin membrane of these materials. periodically the pharynx was replenished by freshly nibbled wood. During the final stages of plastering the membrane, when the aperture was becoming very small, the arrangement of the mouthparts could be seen at times in front elevation, and this permitted one to observe that the wood and silk were admixed for plastering. How far this happens during the initial stages of sealing I did not observe, and it is quite possible that the silk may then serve as a support or cement for the regurgitated particles of wood. When the sealing membrane was being spun the head of the larva described the orbit of the burrow entrance but laid down the material in greater quantity at one region so that the small orifice left toward the end of the operation occupied an eccentric position. As long as it was able the caterpillar used the labrum, so admirably adapted as a smoother and flattener, outside the membrane but when the orifice left for sealing was too small to permit the labrum's extrusion the method of completing the sealing could not be followed precisely but it appeared as if the caterpillar just plugged the hole. The burrow itself has a smooth lining in which silk can be detected and the smoothing of the burrow is most probably performed after excavation and before sealing. When the larvæ were discovered to be wood burrowers, my curiosity was aroused as to what happened in nature because, to the best of my recollection, one wood at least—the moor pinewood—did not contain any soft wood suitable for pupation. Another visit, however, showed that there were fallen trunks in the right condition for boring, but they lay at distances up to and beyond 20 yards away from most of the clumps of fern on which the larvæ fed. The ground, too, between ferns and logs was rough and uneven. These are fair distances and somewhat rough going for larvæ to crawl and, unless they possess an instinct enabling them to find the nearest way—an instinct the existence of which I doubt—they may wander perforce much further before they stumble upon their resting quarters. But in view of the wild energy displayed in the rearing jar it would appear that such a migratory effort from food plant to logs would be within their powers. The rearing experiments, however, show that pupation in litter is possible so it is quite likely that the natural litter of the pinewood would also serve for resting quarters.*

Pupation. The *resting period* for those 1921 larvæ which hatched as imagos in the spring of 1922 proved to be 10-11 months for *mixta* and 10-10½ months for *macula.* In the case of larvæ reared in the laboratory during 1922 and kept there over the winter the resting period would probably have been a little shorter—about a fortnight—because my dissections for cytological purposes made on March 20, 1923, showed the pupæ to be so well-advanced that sperms had left the testes. (The laboratory temperatures, owing to the position and nature of the room were not above those of the outside.) This probability of early spring emergence receives corroboration from the fact that other experimental species, *Pteronidea ribesii and Pristiphora pallipes*—both early spring emergers—appeared a fortnight to three weeks earlier than last year.

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* My friend, Miss E. F. Chawner, makes the useful suggestion that the larvæ burrow into the fern roots.

Deferred Emergence. Certain healthy males of *mixta* whose larval year class was 1921 were discovered changing to pupæ in the larval skin and dissected on March 20, 1923. A single larva of 1921 also was found but as the very small gonads were lost during dissection the sex was not determined. Another larva of 1921 was still alive on July 11, 1923, and, at this late date, is hardly likely to emerge as an imago this year The larvæ of 1922, the offspring of the 1921 collection, were nearly all dissected during March, 1923, but, on July 11, 1923, 6 healthy larvæ were found in wood.

Concerning *macula* a female whose larval year was 1921 hatched on May 3, 1923.

Hence both species may rest an extra year before emerging and one specimen of *mixta* shows every sign of resting two extra years.

Imago. The dates and order of emergence of adults from the 1921 larvæ were as follows:—

1922.	*mixta*		1922	*macula.*
Apr 29	1 female.	1 male.	May 8	1 male.
May 2	2 females		,, 15	1 female
,, 3	1 female.	1 male.	,, 17	1 female
,, 5	1 female.			
,, 6	1 female.			
,, 8	2 females.			
1923.			1923	
Mar. 20		1 male.	May 3	1 female.

These numbers would have been greater but for the facts that certain larvæ were given away, some were dissected and some were parasitised, while the next generation (1922-3) were obtained parthenogenetically. By combining the results of rearings and dissections of the 1921-2 generation I obtained 17 females and 4 males of *mixta*, plus 1 ichneumon, and 3 females and 2 males of *macula* plus 6 ichneumons. From the figures little can be said as to differential sex ratios or times of emergence of the sexes.

Parthenogenesis. Both species are readily parthenogenetic and, with me, arrhenotokous. In the case of *mixta*, 8 pupæ, either in or out of the larval skin were males; the sex of one larva, dissected before the caterpillar burrowed, was not determined; six larvæ, sex undetermined, are still resting July 11, 1923). Twenty pupæ of *macula* were all males. These results are important because they are at variance with that of v. Rossum who, according to Winkler (3) states that from parthenogenetic eggs of *mixta* he obtained 1 female which laid parthenogenetic eggs from which emerged 7 larvæ. Three possibilities suggest themselves to account for these different findings, viz., (1) v. Rossum's laboratory strain may have become contaminated by the addition of a wild specimen which had been sexually produced and introduced with a food plant; or (2) the species is usually arrhenotokous but occasionally produces a female parthenogenetically; or (3) the species, according to locality, may show differential parthenogenesis as in the case of *Trialeurodes vaporariorum* which in England is female-producing and in North America is male-producing.

SUMMARY.

1. New features of the distribution, life history and biology of *Thrinax mixta* are described and an account of the life history and biology of *T. macula* given for the first time.

2. The larvæ of the two species are differentiated.

3 The two species are "parallel species."

4. The method and rationale of the laying of the peculiar eggs are given in detail.

5. The operation of burrowing by *macula* is described in detail.

6 Both species may defer emergence for a year; one specimen of *mixta* shows every sign of postponing emergence for two years.

7. Both species are arrhenotokously parthenogenetic; this result is at variance with v. Rossum's findings in the case of *mixta*.

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THE PHILOSOPHY OF N. O. LOSSKY.

By S. TOMKEIEFF.

[Read February 22nd, 1923.]

Prof. Lossky is the first Russian philosopher to attract the attention of Western thought. This fact alone attests the originality which characterizes his philosophy, and which is recognized as giving it a prominent position in the foremost rank of modern philosophical developments.

Russian thought, a product of the fusion of East and West, from its very beginning proved to be deeply philosophical. But although philosophising in general is an inherent feature of the Russian character, Russia on the whole has not produced any prominent school, and the history of Russian philosophy cannot be compared with those of other countries. Russia did not receive the inheritance of Greek philosophy, nor did she pass through the period of scholastism. Moreover the tendency of Russian philosophy was mainly ethical. The problem of value for a Russian was always superior to that of being.

From the very beginning, Russian philosophers were deeply influenced by foreign sources, but it is not without significance that the first of any importance—G. Skovoroda (1722-1794), was a follower of Neoplatonists and early Christian philosophers. During the great intellectual progress made by Russia in the 19th century, philosophy still bore an eclectic and imitative character. Epistemological foundations were lacking and it was preoccupied mainly with ethical and social problems. Philosophical systems were wholly imported from abroad, to serve as a basis of ethical teaching. They were mainly of a dogmatic character and critical philosophy never got a strong footing in Russia. German idealists, materialists, positivists—all of them had their share of influence. But in the second half of the 19th century arose a more or less original school of Russian philosophy. The forerunner of this movement was P. Yourkevitch (1827-1874), who wrote under the influence of Plato and in opposition to the prevailing tendencies of his own time. V. Soloviov, L. Tolstoy and Th. Dostoevsky wholly expressed the ethical-religious side of the emerging philosophy, although the metaphysical side was almost altogether lacking.

V. Soloviev (1853-1900)—the Russian Newman, as he is sometimes called, was a very prominent figure in Russian philosophy and theology. His profound mysticism, his ardent search for truth and goodness, rank him among such men as Plotinus, St. Augustine and Th. Aquinas. According to Soloviev, knowledge is possible neither through experience alone, nor through reason alone. Neither, by itself, can give us the picture of the real. The real, which is a perfect unity (τὸ ἓν, τυ πρῶτον of Plotinus) is not external to the subject, neither wholly inherent in him. The real is given to the subject by a direct apprehension in all its organic unity. This is what Plotinus calls νόησις or intuitive knowledge. The Absolute, therefore, is not a deduction from perception, nor a construction of our mind, but, "that which is experienced."

Soloviev's philosophy was further developed by S. Troubeskoi and finally taken by N. Lossky as a basis for his epistemological study.

This gives us a very brief summary of the sources of Lossky's philosophy, so far as he found these in the Russian school. On the other hand, we can trace a certain similarity with, if not direct influence from, such western philosophers as F. Jacobi, H. Lotze and W. Schuppe. F. Jacobi was probably a follower of Plato. His "philosophical instinct" is very closely akin to the Platonic "ἔρως,"—a force driving man to seek knowledge, not of the appearance, but of the true Being (Ὄντως Ὄν) or Absolute. This Absolute is not the creation of our thought but our creator. It is immanently present in our mind and not given as a copy of phenomenal knowledge. The basis of all knowledge is therefore an intuitive revelation—the spiritual feeling (*Geistes Gefühl*) and not the partial image of Being (the phenomenon of Kant) or the sense-data of empiricists.

The same intuitive philosophy we can find in Lotze. According to him the undivided feeling is prior to thought, which is merely a formal function acting upon a given content of intuition, or, as Lotze himself expressed it—"the unity of a significant Idea forms the primary datum—an Idea which, from its absolute worth, deserves to be the deepest and most solid foundation of the universe. . . ." The close similarity between Lossky and Lotze can also be observed in their conception of the Universe as an organic whole.

Lossky is probably the first Russian, who disentangled the epistemological-metaphysical problem from its ethical-religious superstructure He is one of the first Russian representatives of pure philosophical thought, but at the same time he preserves in it the best traditions and ideas of Russian ethical philosophy. The very title of his *magnum opus* in its first edition was: "Foundation of Mystical Empiricism," which seems to suggest an attempt of unification of Western empiricism with Eastern mysticism. In a subsequent edition nevertheless Lossky found it necessary to change this title into a less ambiguous one, *i.e.*, "Foundation of Intuitivism" (The Intuitive Basis of Knowledge. Engl. transl.). By the terms mystical and mysticism, Lossky meant only a certain theory of knowledge, according to which the transubjective world is apprehended by the mind as directly as the internal world of feelings and ideas. In popular interpretation, on the other hand, "mystical" means something mysterious, something dark and weird. This popular conception certainly does not apply to Lossky's idea.

In his "Foundation of Intuitivism" Lossky did not attempt to solve ontological problems. He only approached the problem of knowledge, for this he considers to be the fundamental problem of philosophy. A great part of his book is concerned with a critical study of previous philosophical systems. The very able criticism of Lossky opens up the road to his new theory of knowledge, and therefore we have to consider it in some detail.

According to Lossky the fallacy of subjective empiricists (Locke, Berkeley, Hume) consists in the fact that they build up their theory on certain assumptions the true value of which cannot be proved. These *a priori* assumptions are of the following kind:—

(1) That the Ego is completely separated from the non-Ego (I and the external world).
(2) That all contents of consciousness are subjective mental states of the knowing individual.
(3) That experience is the result of *causal* action of non-Ego on Ego.
(4) That the states of consciousness do not present the reality directly but are copies of it.

Building up our theory of knowledge on these assumtions we inevitably arrive at a self-destructive scepticism, which obviously throws doubt on its own validity. This is the "*cul-de-sac*" of subjective empiricism.

Pre-Kantian rationalism also does not avoid certain unproved assumptions and certainly does not bridge the chasm existing between the Ego and the external world. Reality is still presented to the Ego in the form of copies in the mind. Leibniz writes that perception is "an inner state of the Monad which represents the outer things."

Subjective empiricism and rationalism, according to Lossky, agree only in one positive conclusion: "They both assume that the mind can know its own feelings and ideas with perfect adequacy . . ."[1] which means that in this case the known object is immanent in the process of knowing.

Kant produced a revolution in philosophy, but it can hardly be said that he has solved the whole problem. His fundamental assumption is the same as in previous systems: that empirical knowledge is the result of an *action* of the non-Ego on the Ego. According to him the world presented to us is a discontinuous whole and all the relations and meaning are supplied by the mind. "Not only does Kant banish from the world all activity except the intellectual synthesising of the data of sense, he also banishes from the world all

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[1] 2, p. 66.
(The number refers to the list of works of N. Lossky, given below.)

inner meaning and therewith all *inner relation* between its elements."[2] If we developed the idea of Kant to its logical conclusion, we should altogether deny the existence of the external world. But in spite of all its defects, Lossky thinks that Kant's theory, by destroying some of the old fallacies, prepared the ground for the theory of intuitivism.

This new idea seems to be present in some hidden form in all the main tendencies of philosphical thought in the 19th century, but at the same time it appears that it was the chief cause of their disagreements, rather than of their unity. According to Lossky, the mystical rationalists such as Fichte, Schelling and Hegel "did not realize that the new principle merely justifies the assertion that there are no *insurmountable* obstacles to a knowledge of the world, but affords no grounds whatever for regarding human thought as divinely omniscient."[3] They were too hasty in their solution of the Riddle of the Universe and wanted to embrace the infinite. The same also can be said about their antipodes—the positivists and the materialists of the 19th century, who attempted to build a Universe on the basis of detached sense data or certain abstract ideas like "matter" or "energy."

In opposition to his predecessors, Lossky approaches the subject of knowledge, not from the side of perception or the object, nor from that of a general *a priori* idea, but by trying to understand the process of knowledge in itself. He does not think that we know a thing, only when we can describe it, or find relations between its elements. This he supports by a quotation from Gœthe's Faust:

> "He who would study organic existence,
> First drives out the soul with rigid persistence;
> Then the parts in his hands he may hold and class
> But the spiritual link is lost, alas!"

Every positive science proceeds in this way. This does not diminish its practical value, but epistomology, being the fundamental basis of science, cannot be built up on the results attained by certain special sciences. Those results can be introduced only as a *material* and not as a *foundation* for a theory of knowledge.

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[2] 2, p. 128.

[3] 3, p. 161.

"In other words, a theory of knowledge must begin with an analysis of the experiences actually taking place at any given moment.[4] Any dogmatic assumption, even the division between Ego and non-Ego, must be abandoned in approaching this problem.

And so, if we discard all our preconceived ideas about the Universe and confine our attention to a simple manifestation of our consciousness as expressed by any judgment of perception, as for example: "I am glad," "I see a house," etc., we always find in it something that stands in a certain relation to ourselves. This something Lossky calls "given to me," and this "given to me" passes into that which is "mine." We usually make a distinction between the things observed in the external world and our own ideas and feelings. The latter we call "ours" and we are sure that we perceive them as they are. Lossky thinks that there is no difference in this subject between the perception of the "given to me" from the outside, and the "given to me" from the inside, so to say. Every thing "given to me" becomes a "content" of my consciousness. My psychical states may be given to me just as much as the object of the external world. "In this case they will stand in a twofold relation to me. They will be 'mine' (in the sense of being a state of myself) and they will be 'given' *to 'me' for observation*."[5] Therefore the fact of consciousness involves at least three elements:—

(1) The self.
(2) A content.
(3) A relation between the two.

As we saw before, the "content" of consciousness is formed by everything "given to me," *i.e.*—standing in a certain unique relation to me. This relation is not a casual one and is simple, so that it cannot be defined or described. Using the modern logistic terminology, we can assign to this relation the properties of being a one-one, non-symmetrical relation. "We will call this unique relation between the Ego and something which owes its existence to my attention and leads to that something "given to me" *perception*, *intuition*, or *gnoseological co-ordination*, between subject and object "[6]

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[4] 2, p. 14.

[5] 8, p. 129.

In a later edition of his book, Losky finds it necessary to distinguish between those terms According to his new definition, gnoseological co-ordination is a super-spatial and super-temporal relation between the Ego and the object and it is prior to the intuition On the other hand, intuition is the act of awareness, which becomes possible only in virtue of this relation [7]

To have something in consciousness (or in mind, to be more precise), a simple awareness of something, certainly is not yet knowledge "A content of consciousness may be unknown, either completely or at least in so far as I am not able to describe it "[8] When, for example, we are looking at something unknown to us, our first impression is an indefinite, obscure feeling of something presented to our consciousness But gradually we begin to discriminate certain aspects of the given, and we are comparing those aspects with something we know already, something present in our memory But "*the entirety of a thing is prior to its elements* "[9]

According to Losky the judgment of perception is the result of the differentiation of the "given" by means of comparison The same idea we can find in Bradley's *Appearance and Reality* For him the judgment of perception is composed of two elements "that" and "what" "That" and "what" respectively, correspond to the image and the idea about the image They can never exist in our mind separately, but are always united At the same time they cannot be said to be identical The copula —*is*, does not represent an equation "In judgment an idea is predicated of a reality," said Bradley (App and Real p, 163) But the predicate is not merely a mental idea or another image, it is a quality of the very reality that we are contemplating "Judgment is essentially the re-union of two sides, 'what' and 'that,' provisionally estrangled." (Bradley, p. 165.) It is very difficult to assert with any conviction, that Lossky was in any way influenced by Bradley in his view on the nature of judgment, most probably he was not, but the similarity between both is striking. For example, Lossky writes: "The object known is the real world. But since everything about which we judge is real in one sense or another, we want to know not *that* an object is, but *what* it is—quid sit, and not quod sit." (*Int. Bas. of Know.*, Engl. transl., p. 261.) The terms "that" and "what" of the last sentence are introduced by the translator, probably for the sake of clearness. The literal translation of the sentence would run as follows: ". . . . not that the object *exists*, but *how* it exists—not quod sit, but quid sit."

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[6] 4, p 245 [7] 3, p 133 [8] 3, p 135 [9] 7, p 176

The predicate of that-what judgment is a concept. Croce, like Bradley, derives the pure concept from the Reality itself. The expression of this concept, according to Croce, constitutes what is commonly called the "definitive" judgment. This judgment is formally different from the individual judgment, forming the perception in its strict meaning. Perception is pre-supposed by the concept formed in the mind by means of the definitive judgment. "To perceive means to apprehend a given fact as having this or that nature; and so means to think and to judge it."[10] Lossky also shows that there are different kinds of judgments and "that if several successive acts of knowing are directed on one and the same object, they will result in the formation of a concept as well as of judgments."[11] But he is not quite clear about the distinction between the perceptual "image" and the "given reality." For my own part I think, that the terms "intuitive perception" or "intuitive knowledge," must not be used, for they lead to a misconception. The term "knowledge," in any case, must be used as Lossky defines it himself, that is as a process of analysis of reality by means of comparison. Knowledge is pragmatical in its nature and can never give the *whole* truth, for reality can be subdivided into an infinite number of aspects. In this respect we are in a perfect agreement with Croce, who says that "it must be firmly maintained that the first moment of knowledge is *intuitive* and not perceptive; and that the concepts do *not originate* from the intellectual act of perception, but enter the act itself as constituents. To begin with perception, understood as perceptive judgment, is to begin at the end, that is to say with the most highly complex."[12]

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[10] Croce. Logic as Science of the pure concept, p. 115.

[11] 8, p. 130.

Except in the matter of terminology and exact definition of terms, the analogy between Bradley, Croce and Lossky on this subject is very striking. From his theory of judgment, Lossky draws the conclusion that every judgment is an analysis of the unknown part of reality, and at the same time a synthesis with reference to the known parts of the reality. Bradley expresses the same idea in the following manner: "For judgment is the differentiation of a complex whole, and hence always is analysis and synthesis in one."[13]

In Croce's work we can see a more elaborate statement of the same idea: "If analysis apart from synthesis, the *a priori* apart from the *a posteriori*, be inconceivable, and if synthesis apart from analysis, the *a posteriori* apart from the *a priori*, be equally inconceivable, then the true act of thought will be a synthetic analysis, an analytic synthesis, an *a posteriory-a-priori*, or, if it be preferred, an *a priori synthesis*."[14] This is a return to one of those ideas of Kant which prepared the way for intuitivism.

From all this it follows necessarily that a true judgment must be objective (transubjective). It must not be a copy, or a bundle of copies of reality, but it must itself be contained in reality. "In this sense it can be said that truth is an objective image of a thing, and falsity a subjective image of it."[15] A true judgment is therefore eternal, necessary and universal. In other words, a true judgment is present *a priori* in reality. What about the subject—is he not a part of reality also? Is it possible that an *a priori* judgment can be formed in a togetherness of the subject? Lossky does not go so far as to admit this, and the problem remains unsolved. The relation between the Ego and the World is the deepest mystery of the universe, but, as the poet said—

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[12] Croce. Logic, p. 159.

[13] Bradley, Appearance and Reality, pp. 169-9.

[14] Croce. Logic, p. 218.

[15] 3, p. 137.

> " The creature is in Brahma, and Brahma is in the creature: they are ever distinct, yet ever united."
> (Kabir, *One Hundred Poems*).

But one thing concerning the judgment we must clearly keep in our mind: a judgment may be a true one, but it may not be the whole truth about the reality. In every act of judgment we analyse the given reality by abstracting some of its aspects and always leaving an undifferentiated residue. A given reality as we know can be subdivided into an infinite number of aspects, and therefore no finite sum of judgments about it can express the whole truth.

According to Lossky the relations in judgments are not produced by the subject, they are only discriminated by him. The world is gradually opening up to the knowing mind—the mind gains a hold on reality and unfolds it. Appearance is the perceived reality, or abstracted and compared aspects of reality. Appearance belongs to reality, and in this way the chasm between appearance (knowledge) and reality (being) is bridged.

Some objections may be raised in connection with the nature of indirect judgment. Usually we do not acquire our knowledge by means of direct judgments. " The sun is rising "—is a direct judgment when we observe the sun rise above the horizon, but do we accept the value of this judgment in astronomy? No science would be possible without indirect judgments and indirect inferences. " If such inferences be merely probable, almost the whole science of geology becomes a system of hypotheses, the truth of which can never be established by any progress of knowledge. Exactly the same would have to be said of history when it recreates the past from the ruins of old towns, from inscriptions, coins, etc."[16] Lossky tries to solve this problem by extending the meaning of " givenness." The given data may be of two kinds: sensuous or non-sensuous (supersensuous). As we indicated before, even the ordinary perception requires supersensuous data —*i.e.*, concepts. "The more we seek to know the external reality as it is in itself, and not in its effects upon our body, the more we strive to abstract from the sensuous data and to let the non-sensuous elements of experience enter into the foreground. The *knowledge which consists in apprehension of the non-sensuous* I propose to call *speculative knowledge, or intellectual intuition.*"[17] From this it follows that every kind of knowledge is in a certain degree speculative. According to Lossky, the non-sensuous data, including the universals, are also in the reality and given directly to the subject. On this point I disagree with Lossky, and without being afraid to be called a nominalist, I think, that universals are inferred from reality and do not exist in the same way as particulars may be said to exist. We can say that "tallness" exists, for we speak and think of it, but its existence is somewhat different from my own existence or from the existence of a particular tall being. To posit the universals as existing at the same level as the particulars is a step towards an atomic new-realism. I think that universals depend on particulars and are a result of our thought, a kind of mnemonic device for the economy of thinking. Certainly this idea is purely nominalistic, and as we know, nominalism is considered somewhat akin to materialism. Nevertheless, in my opinion, nominalism is not out of place in the theory of intuitivism. The universals may be taken out of the reality but their moment is subsequent to that of the particulars. We may perhaps agree that *universalia* are *in re* in the reality taken as one whole, but they are certainly *post rem* when we are thinking about its isolated aspects. Lossky writes that "universals have real being and are given in perception."[18] I should prefer to state it this way: universals are given in intuition of the real *en durée*. The addition of the time element, or the Bergsonian *durée* is, I think, necessary for the thought process involved in the apprehension of universals as such. We may disagree about the objective or subjective nature of time, but that time exists we are all sure. It may, if we like, be regarded as an intransitive relation of a serial order The process of thought occurs in time—*en durée*. The mind *endures* reality with the formation of a Mneme, that is the sum-total of its past states. The Mneme in its turn, acts upon the present and produces the future. For certain purposes of dialectic discussion we can abstract an instant of intuition or an instant of perception regardless to the time element, but this abstraction can never reveal the real process of thought and the origin of concepts. I do not see how we can approach the problem of universals and of indirect judgments, leaving out of account the elements of *durée* and Mneme. The way of formation of concepts can be illustrated thus:—when we perceive a new aspect of reality—" this thing," we always compare it to something else, usually presented to us by memory. We cannot think of something presented to us without thinking the same time about something else. The judgments of perception are built up, linking the unknown aspects of the given with some known familiar aspects existing in our Mneme. Suppose we perceive the same aspect of the given reality again and again or even imagine it repeatedly. This process has to proceed in *durée* by means of Mneme. The essential property of Mneme is that the often repeated process become, so to speak, canalized and condensed for the purpose of economy of thought. This condensation of aspects of perception leads to the formation of concepts, a kind of shorthand designs for the apprehension of the variety of experience. Certainly no pure perception is ever possible, even in a new-born infant, because every organized being possesses a mnemic stock in virtue of its organisation.

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[16] *Ibid.*, p. 350.

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[17] 2, p. 264.

[18] 2, p. 368.

This necessarily leads us to the consideration of a wider question of the " self " or individual in the scheme of the universe. Unless we consider the individual as an historical being, that is to say, in its *durée* together with its stock of Mneme, the problems concerning the indirect judgment, the indirect inference and the formation of universals, as presented by Lossky are completely insoluble For what is an individual, if not a product of the past interrelated by innumerable ties to everything outside its own limited existence "Life is the gathering of waves to a head, at death they break into a million fragments each one of which however, is absorbed at once into the sea of life and helps to form a later generation which comes rolling on till it too breaks '[19] Butler's idea of the vicarious nature of human individuality and its progress in the flow of time, must be clearly borne in mind while discussing any gnoseological questions The vicarious nature of consciousness and experience is also propounded by S Troubeskoi and he expresses it in the following words ' human consciousness is not my personal function, but a collective function of the whole human race I think also, that human consciousness is not an abstract term indicating separate individual consciousness, but that it is a living and concrete *universal* process"[20] For Troubeskoi an individual mind is a manifestation of a "*cosmic*" consciousness and a "*cosmic*" memory No isolation between Ego and non-Ego is possible—they are both closely interrelated

If we accept the views of Butler and Troubezkoi on the nature of the Ego then the problem of universals and indirect judgments is more easily soluble The terms of an indirect judgment may not be "mine" in the narrow meaning of "mine ' but somebody else's But this transubjective experience just the same as "mine" experience forms a part of a wider experience of a superhuman Monad which we may perhaps call God Our mind is a creative unity, existing in a process of development in which the past is constantly building the future but what we call our consciousness is an *emergent* quality of the higher Monad or a manifestation of the Whole to use the term proposed by McTaggart[1]

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[19] S Butler, Note Books p 15

[20] S Troubeskoi, On the Nature of Human Consciousness

[1] McTaggart, The Nature of Existence p 121

Although Lossky does not bring in the idea of vicarious personality, his views as regards the meaning of the world process, is closely similar to the conclusions reached above. " It will readily be noticed that we are specially inclined to describe as rational all those activities, and generally all those aspects of reality which we believe to possess a higher meaning, a high super-individual significance—*i.e.*, a significance, to a greater or less degree, for the world as a whole. But significance and meaning are only possible where *there* is purpose; and, *if* so, individual things can have a super-individual significance only if there is purpose in the universe as a whole. Consequently the word *reason* ought to denote the faculty of putting before the mind and of realising these supreme or highest purposes.''[2] Knowledge or reason, therefore, is purposive in its character; in other words it is practical.

The world of which the individual takes part, must possess a complete organic unity, since it has a definite purpose. " In contradistinction to individualistic empiricism and in agreement with rationalism, the intuitional theory lays particular stress upon the organic living unity of the world.''[3] This idea is more developed in Lossky's " The World as an Organic Whole.'' It would also be as well to mention the excellent exposition of the idea of an organic whole, or organic unity, in the recent book by McTaggart (The Nature of Existence), in which he writes that in an organic unity " each part is regarded as determined by the whole which is not merely compounded of the parts, but is manifested in them. . And the relation of each part to the others is that it is not only determined by them, but that it co-operates with them in manifesting the whole.''[4]

The ontological idea of Lossky, as we can now see, is not only akin to that of Lotze, Bradley and Hegel, but also to that of J. Th. Merz. Dr. Merz's " synoptic " method of approach to the world, his dislike of partial views, his belief that " the ultimate reality is not to be reached by thought but must be left, lived, and experienced,"[5] are probably well known to everybody

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[2] 2, pp. 411-12. [3] 2, p 413.

[4] McTaggart, The Nature of Existence, p. 106.

But the unity of the world does not consist in the way of thinking it The reality presented to us is a highly complex substance, the product of a certain creative synthesis, the work of somebody outside our own self We are only forming a part of the world, but not a fragmentary world, full of bloodless entities in a mad dance, but a living organic unity, permeated by an inward meaning and higher purpose As we are a part of the world we also must have a purpose which attains its fullest realisation in the ethical life Being part of the world and at the same time—the heirs of the past, we must strive to contribute as much as we can to the world progress In other words, our highest moral duty our greatest approach-road to immortality, are the things that we are doing for the rest of the world, our life in others " We had better live in others as much as we can if only because we thus live more in the race which God really does seem to care about a good deal "[6] This is the greatest idea that ever existed among mankind and which was always present in all religion and philosophy and is, according to Walt Whitman, the " base and finale too for all Metaphysics '

" The dear love of man for his comrade, the attraction
    of friend to friend,
Of the well-married husband and wife, of children
    and parents,
Of city for city and land for land ' [7]

According to Lossky everything existing is bound up in one perfect unity " Hence it follows that cosmic unity pre-supposes a substantial agent which embraces everything that exists in the universe and binds it together into one ordered whole Such an agent is a higher principle of cosmic meaning and rationality, than the Abstract Logos —it is the Concrete Logos, a living, personal being "[8] Only

[5] J Th Merz, A History of European Thought in XIX century, p 789

[6] S Butler, Note Books, p 15

[7] W Whitman, Leaves of Grass

[8] 12

living in others we can wholly realise our higher purpose and attain our true immortality.

It would be a very difficult task indeed to attempt to place Lossky in a definite school of philosophy. It seems that he selects and unites in himself elements belonging to different schools. In one sense we may call him an idealist, but at the same time we cannot ignore the fact that he rejects the notion that appearances are mainly deceptions screening the real being. In another sense he may be called a realist, but not an atomic new-realist, not one who takes sense-data, qualities and categories as existing independently in the world. In his view on knowledge he is a pragmatist, in his view of art he in part resembles Croce. At first sight one feels tempted to compare him with Bergson, but the similarity between them is not complete enough to allow them to be classed together. In one of his books,[9] Lossky criticizes many sides of Bergson's philosophy. Lossky is in perfect agreement with Bergson's view that intuition gives us an immediate knowledge of an object, but he thinks that the methodology of his system is not quite perfect, because Bergson approaches the question of knowledge partly from the metaphysical, partly from the physiological point of view. He also agrees with Bergson that the world is an organic whole in a process of development, but he holds that reality is not only a flow of change: this is only one of its aspects. Besides change it contains also something that changes. The analysis of mind does not destroy the object—it only abstracts the needed parts, whereas Bergson opens up an impassable gulf between intuition and intellect, and thinks that scientific knowledge is only a symbolic representation, a copy of the real. Science and metaphysics are separated by Bergson; Lossky tries to reunite them. A consideration of these points shows that Lossky and Bergson are too different to be classified together. Bradley, Croce and Hegel certainly have more in common with Lossky.

An attempt to elucidate the question of a system of metaphysics is made by Lossky in one of his latest articles published in the new Russian philosophical journal "Mysl" ("Thought") According to Lossky, the only possible system of philosophy is an ideal-realism, i e, a system asserting that the basis of the spatio temporal world is formed by an ideal being But an ideal realism can be founded on two kinds of ideas, very dissimilar in their essence On the one hand, we can postulate abstract ideas, such as equality, number, quality, etc, on the other, concrete ideas, which are actually present in the real world, such as the Nous of Aristotle, the Monad of Leibniz, Mind, Spirit, etc

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[9] 10.

The only possible system, according to Lossky, is the one based on concrete ideas, and for such a system he proposes the name of *concrete ideal realism* "Every form of ideal-realism must tend to an *organic* aspect of the world The whole must determine its components and not be merely their product [10] An abstract idea is not an idea of an organic whole, not an idea of being, but only a quality of a class or mechanical assemblage of objects Therefore an abstract ideal-realism cannot be taken as a basis of an organistic system of the world And further, abstract ideas are static in their essence and can never give a picture of a dynamic universe as we see it The universe tends to desintegrate into isolated and unconnected "things in themselves," when the categories are supplied by the perceiving mind In this sense the philosophy of Hegel is a true concrete ideal realism But certain theories, such as the panlogism of Cohen, can never satisfactorily explain the category of purpose, so essential for the organic structure of the world Without purpose the world would appear only as a mechanical system, dead and uncreative

An abstract idea can never furnish unity to the system to which it relates Abstract ideas are mere abstractions and not factors The conception of a higher Being standing above any rational or empirical knowledge, was put forward by Plotinus and his follower, V Soloviev This Being has the quality of an *eternal supermindness;* but at the same time It is manifested in every part of the world, including ourselves. We feel Its presence, but cannot think about It in a logical way. This intuitive awareness of this higher Being is the true foundation of our "self." "The concrete ideal realism postulates the existence of a being which dominates, not only the spatio-temporal process, but our own ideas. The ideas are always *dependent moments* of this higher being as, for example, qualities, properties, *capacities*, *possibilities* or *forms* of its activity."[11] Therefore, the logical laws do not apply to this concrete ideal substance—it is a *metalogical* being.

The Absolute is one, undivided, eternal. The apprehension of Absolute is given to us directly, and not through some rational or empirical methods. This direct awareness of the higher being is called by Lossky *mystical intuition*.

All this certainly does not exclude the possibility and necessity of an analytical knowledge, because every aspect derived by means of analysis, is part of the Absolute, and its relations are actually present in the Absolute.

The human soul or mind can be taken as an example of a concrete ideal being. Besides its conscious thought and activity, the human personality possesses an infinite content and infinite potentiality. The human personality cannot be brought down to the level of physico-chemical world, obeying the law of cause, in a mechanical sense of this term. "Those who will accept the existence of concrete ideal entities, similar to the human personality, cannot reduce causality only to the *order* in time."[12] A cause of an event cannot be a detached event preceding it, the real cause of changing appearances is always a concrete-ideal entity underlying every process. This entity is super-temporal, always flowing, always creating and carrying all the past within itself. And all those entities "stand in a relation of co-ordination to each other and to all the elements of the world."[13]

The theory of concrete ideal-realism gives us a biological aspect of the universe. According to this theory every organic whole is composed of parts bound together and co-ordinated by a general purposive activity, which is the expression of its "I." Mind or soul are the names

[11] 12. [12] Ibid. [13] 13.

given to this active entity The term " psychoid " can be applied in case of lower forms of life

" An organism is an infinite machine i e a system in which the organization possessed is overwhelming capable of infinite extention yet complete in character, it is *a concrete purposiveness and concrete organization* "

The theory of concrete ideal realism regards the whole universe as a living organic being Lossky does not push the theory to its logical conclusion but I think that he would undoubtedly agree with Butler when the latter says that The division between varieties species genus all gone between animals and plants gone so ere long the division between organic and inorganic will go and will take with it the division between mind and matter [14]

S Tomkeieff

THF WORKS OF N O LOSSKY

1 —The fundamental principles of psychology from the point of view of voluntarism (in Russian) Petrograd 2nd ed 1911

2 —The Foundations of Intuitivism (in Russian) Petrograd 1906 2nd ed 1908

The Intuitive Basis of Knowledge An epistemological inquiry (English translation by N Duddington) 1919

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6 —Introduction to Philosophy (in Russian) Petrograd 1918

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8 —Logic Part 1 (in Russian) Petrograd 1922

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12 —Concrete and abstract Ideal Realism (in Russian) Myśl No 1 2 Petrograd 1922

13 —Some tendencies of modern epistemology Lecture delivered at the University College London March 1923

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[14] S Butler, Note-Books, p 78

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